



L. R. 1.

THE
Edinburgh
JOURNAL OF SCIENCE,

CONDUCTED BY

DAVID BREWSTER, LL.D.

F.R.S. LOND. AND EDIN. F.S.S.A. M.R.I.A.

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OF SCIENCES; OF THE ROYAL SOCIETY OF SCIENCES OF DENMARK;
OF THE ROYAL SOCIETY OF GOTTINGEN, &c. &c.

VOL. V.

NEW SERIES.

APRIL—OCTOBER.

THOMAS CLARK, EDINBURGH:
T. CADELL, LONDON.
AND MILLIKIN & SON, DUBLIN.

M.DCCC.XXXI.

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THE
EDINBURGH
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ART. I.—*Observations on the Decline of Science in England.*

FOR several years we have availed ourselves of every proper opportunity of pointing out to our readers the deplorable condition of English science, and the rapid decline of the scientific as well as the useful arts. The language of admonition and of expostulation was too feeble to make any immediate impression, but the smallest impulse may produce a sensible effect by its repetition; and we have reason to believe that many individuals were induced to turn their attention to the subject, and were led to discover within the circle of their own knowledge the most palpable evidence of the bitter and mortifying truths which we were the first to acknowledge and to bewail. A posthumous work of Sir Humphry Davy's contained opinions in unison with ours, and Mr Herschel felt himself called upon not merely to assert in general terms that the sciences of England were declining, but to bring forward specific and incontrovertible proofs of the fact.

“In England,” says he, “whole branches of continental discovery are unstudied, and, indeed, almost unknown even by name. It is in vain to conceal the melancholy truth. We are fast dropping behind. In mathematics we have long since drawn the rein, and given over a hopeless race. In chemistry the case is not much better. Who can tell any thing of the sulpho-salts? Who will explain to us the laws of isomorphism? Nay,

who among us has ever verified Thenard's experiments on the oxygenated acids? Oersted's and Berzelius's on the radicals of the earths? Balard's and Serullas's on the combinations of brome, and a hundred other splendid trains of research in that fascinating science? Nor need we stop here. *There are, indeed, few sciences which would not furnish matter for similar remark.* The causes are at once obvious and deep seated; but this is not the place to discuss them."

This powerful statement, which no person has ever dared to controvert in the minutest particular, was followed up by Mr Babbage's bold and able work on the Decline of Science, of which we have already laid before our readers a full account.

A review of this work appeared in the 87th Number of the *Quarterly Review*, where the writer not only defended Mr Babbage's views, but adventured upon the dangerous ground of pointing out in the most fearless manner the causes which have led to the decline of science. He exposed the infamous and fraudulent system of our patent laws, and explained the improvements in our universities, in our metropolitan institutions, and in our public boards, by which the genius of our countrymen might still be called forth and cherished, and the scientific glory of England preserved and extended.

The metropolitan and the provincial press took up the subject with their usual boldness and intelligence, and the facts of the case were speedily diffused over all England. Two parties were of course arrayed against each other. Men of science who had pursued it for its own sake, and who had distinguished themselves by original discoveries, entered the ranks with Mr Herschel, Mr Babbage, and Sir James South, and were supported by many of the intelligent and disinterested friends of science; while another party, which we can scarcely characterize, was carrying on an anonymous warfare in the bye-ways of our periodical miscellanies. Individuals of no pretensions even to science, and who had all their lives thought that there could be nothing either good or great out of England, considered it as next to treason to state that the French excelled us in mathematics,—the Swedes and the Prussians in chemistry,—the Russians in practical astronomy,—and the Bavarians in telescopes. The functionaries of scientific institu-

tions that had long been identified with English science, felt themselves aggrieved by the mere allegation that science was declining, and that their labours were gradually diminishing in importance; and certain professors in universities, who had never enriched science with a single fact, and yet considered themselves as its guardians and depositaries, have raised a violent outcry against facts which all but themselves knew, and against opinions which all but themselves believe.

Cutting off the auxiliaries from both sides, we may safely state, that the parties arrayed against each other on the question of the decline of science, and the unfitness of our present institutions to advance its interests, are certain professors in our universities on the one side, and the philosophers actively and constantly engaged in original research on the other. Both of these classes of persons are of the highest respectability, and exercise a great and a just influence over public opinion; and those who are led to form their judgment chiefly from the authority of names will have some difficulty in taking their place in the controversy, and will probably arrange themselves under the banners of those men who enjoy a temporary and deceitful reputation from their influence as teachers of youth. Many of these persons never heard of Mr Ivory and Mr Dalton, and would scorn the idea of their being greater men than some of our well-known and justly esteemed professors. We must therefore furnish the intelligent reader who wishes to form a correct opinion, with a test by which alone he can decide which of the two parties are most likely to form the most correct judgment respecting the decline of English science.

Original discoveries in science are rewarded in two ways, either by the adjudication of prizes or medals to the discoverers, or by honorary admission into foreign societies.—Among the living cultivators of science in England, *twenty-four* honorary medals have been adjudged by the Royal Society of London; viz. nineteen Copley medals, given chiefly for the best papers in each volume of the *Philosophical Transactions*; two Rumford medals for discoveries on light and heat; and four or five royal medals for specific discoveries. Now it is a curious fact that only a single professor received one of these medals, * viz. Pro-

* Mr Leslie, before he was a professor, received the Rumford medals, and Dr Brinkley received a Copley medal as Astronomer Royal of Ireland.

fessor Buckland. Several medals have been awarded by foreign societies to English philosophers, but none of them have found their way to any of our universities.

With regard to the honorary distinctions conferred by foreign academies, we may state without the fear of contradiction, that the Institute of France is the only body which dispenses its honours with rigid impartiality. No adventitious circumstances are permitted to bias their decisions; and if a few votes should on particular occasions be influenced by personal feelings, they are entirely lost in the unanimity which invariably characterizes the election of associates and corresponding members. Now there are at present in England *nineteen* corresponding members of the Institute of France, *and not one of them is a professor, or even a resident in any of our English universities.*—Professor Leslie, indeed, is the only one who fills a chair, but he had earned this honour before his brilliant talents were lost to science in the functions of a popular lecturer.

It will not be imagined by the candid reader that we mean to call in question the genius and talent of many eminent professors who adorn the universities to which they belong. We appreciate fully their valuable labours as excellent teachers and eloquent lecturers, and we see among them more than one individual who has begun to earn a brilliant reputation, and who, if not overpowered by professional duties, will add to the scientific glory of their university and of their country. Professors Airy and Whewell of Cambridge, and Professor Hamilton of Trinity College, Dublin, are the individuals to whom we more particularly refer, though there are still others to whom we look with a feeling of hope, which we trust will not be disappointed.*

The great fact, therefore, of *the decline of science, and the necessity of a reform in its institutions*, are maintained almost unanimously by the individuals who stand at the head of English science, and who represent it in foreign countries, and by a host of their distinguished fellow-labourers, many of whose names are recorded in the celebrated requisition signed by about eighty fellows of the Royal Society, which

* Our remarks have no reference to the sciences of observation, such as geology and natural history.

called Mr Herschel to become a candidate for the presidency of that institution. Their opponents are the occupiers of offices and chairs in the close burghs of our universities, who, as we have already proved, have not contributed their proper share to the scientific glory of their country, and who are thriving on the luxuries of pluralities and official emoluments. These individuals have no sympathy for the condition and hardships of their scientific brethren, who, to their ardour for science, have sacrificed all the advantages of professional ambition, and who, in the nightly bivouac of a campaign that never ends, are exhausting their minds, their healths, and their limited means in advancing the interests of their species, and contributing to the intellectual glory of their country. The privileged philosophers believe, that in place of science declining, England surpasses all nations in her scientific men and in her artists; that our philosophical institutions are nearly perfect, and that any attempt to improve them would detract from the wisdom of our ancestors, without adding to that of our posterity.

In these views we recognize the arguments which have been used against any—even the smallest and the slowest improvements in our political institutions; and it is curious to discover that the same persons who are opposed to a constitutional reform in our representative system are in general equally hostile to a reform in our scientific institutions. Sir Robert Peel has on more than one occasion expressed a disbelief in the decline of science in England, and denied that it has not been sufficiently encouraged by the government. Such opinions are a defence of his own conduct as the Home Secretary of England. In this high situation, it was his duty to observe the downfall of English science, and to recommend measures for its recovery. Representing as he did a learned university,—deriving as he did, and as he himself with much good taste declared,* all his importance in society from the useful arts,—canvassing as he, or his friends did for him, for the presidency of the Royal Society; and entering as he did the council of that body,—a peculiar obligation was laid upon him to promote the scientific interests of England, and to improve the vicious system of her patent laws. The force of this obligation he could not but feel, and the very

* Speech at the meeting for erecting a monument to Mr Watt.

proofs which he has given of this feeling betray his unwillingness to advance the interests of science. He recommended to his late Majesty to give L. 100 per annum for two royal medals, which are adjudged not to Englishmen alone, but to philosophers in every part of Europe; and he advised his present Majesty to give L.300 per annum to Sir James South, a gentleman of great wealth, in order, to use his own words, “that the country should bear *some portion* of the enormous expence which Sir James had incurred in pursuing his researches!” and in order “*to relieve the country from the charge of perfect indifference to subjects of a scientific nature!*” While the other governments of Europe are building magnificent observatories, and purchasing the most expensive instruments for promoting astronomy, a British minister recommends to his munificent Sovereign to pay *some portion of the enormous expences of a wealthy astronomer*, and never feels it his duty to propose any act of liberality to other men who are involving themselves and their families in pecuniary difficulties by the noble devotion of their entire minds to the intellectual service of their country. But the British minister has another motive for this Irish act of liberality, namely, the motive of self-defence against the charge of perfect indifference to subjects of a scientific nature. Which of these motives predominated and prompted the recommendation, we dare not conjecture; and, however others may think upon the subject, we cheerfully admit that the grant to Sir James South relieves the country from the charge of *perfect* indifference to science, in the same manner as the miser would sustain his character for charity, by passing by the poor man at his gate, and casting a mite into the treasury of the rich.

In thus totally neglecting the scientific interests of his country, Sir Robert Peel has only followed the example of his predecessors in office,* and we have given our observations an especial reference to him, because his conduct is best known to our readers, and because being the last occupier of the Secretaryship of the Home department, and living at a time when the diffusion of knowledge had ceased to terrify weak minds, the

* We beg to refer the reader to the Life of Dugald Stewart in this *Journal*, No. xx. p. 200, for an anecdote of Lord Liverpool, strikingly illustrative of this opinion.

intellectual population of England were entitled to expect from him a more generous and enlightened policy. When the politics of D'Alembert, Condorcet, Carnot and Voltaire, were quoted as arguments against science and literature, there might be some apology for an ignorant minister when he yielded to the fears of his still more ignorant abettors; but in the days of Sir Robert Peel, such fears as these, if they existed at all, were to be found only among men defective in reason, and could not even be felt, still less urged, as grounds of hostility to the cultivation of science. The true cause of the indifference of the government was this; the ruling party in Parliament did not contain one single scientific individual, or one single patron of science. There was therefore no influential channel through which scientific claims could be preferred; and in proof of this we can state on undoubted authority, that when application was made to government for a grant to construct a great national machine, the minister durst not give his consent till he had obtained the concurrence and recommendation of the eminent philosopher and statesman who was the leader of the opposition. But it was not merely ignorance of science and of its value to a nation that was the cause of this evil. Corruption and ignorance lay with their mingled poison round the tendrils of the tree of knowledge. The fund with which the Sovereign was entrusted for *rewarding merit* was all required for the purposes of parliamentary corruption, and the offices which men of talent could have filled with honour and utility, were all reserved for the tools of faction.

In reply to these observations, it is usual to adduce some acts of liberality and justice. Is not such a scientific and literary name on the pension list? Was not L. 12,000 or L. 15,000 given for a mineralogical survey of Scotland? Did not several expeditions of discovery sail from our ports? To these questions we answer in the affirmative: But if we were to inquire into the real history of these transactions, we should find that many of them were the merest jobs, though the avowed object of them was praiseworthy; while others were the tricks or expedients of a designing minister, to throw round his measures a thin halo, sufficient to conceal from the people his own scheme of personal aggrandisement. What is it to science that two medals are given annually by the Royal Society in the name of

the King of England to a British or to a foreign philosopher? What is it to science that a wealthy individual receives L. 10,000 for a scientific survey, which could have been executed for L. 1000? or that several expeditions, got up avowedly for a scientific object, but really for the benefit of naval men, have left our ports without a single astronomer, hydrographer, or naturalist on board? But though such fragments of a shallow and deceitful policy are nothing to science, yet they are everything to a minister. He pleads upon them as points of character, and when his insincerity is unmasked, and his miserable expedients detected, he consoles himself, and blinds his friends, by the proof that he was not perfectly indifferent to the great interests that were confided to his care.

The object of these observations is to introduce to the reader the opinions of an able cultivator of science, resident in India, respecting the comparative condition of the sciences in England and France, and respecting the decline of science in England. These opinions are given in an excellent work published at Calcutta, entitled *Gleanings in Science*; and we believe we need not scruple to say, that the author of them is Captain Herbert, assistant surveyor-general of India, and late superintendent of the mineralogical survey of that country. To the sentiments which this disinterested and able writer has expressed, we would beg the particular notice of our readers, especially of those who may be disposed to entertain an opposite view of the subject; and we would direct his particular attention to the absolute coincidence between Captain Herbert's views and those of Mr Babbage, Mr Herschel, and the writer of the article on the decline of science in the *Quarterly Review*.

“ If to the labours of the officers of *La Chevrete* we add those of MM. Diard and Duvaucel, of M. Dussoumier, and of a gentleman well known in Calcutta, now busily employed in investigating the natural history and physical geography of India, we shall be forced to confess, however humiliating the acknowledgment, that France will have done more in the short period of the peace for making India known to the scientific men of Europe, than England has in the whole period during which she has held the country. There is a general spirit of scientific research diffused throughout the French nation, which

can only be attributed to the great interest taken in these inquiries, and to the encouragement afforded to their pursuit by the French Government. In England a very different state of things prevails. There, with a few occasional exceptions, every thing is left to private emulation and the promptings of commercial enterprise ; so that those branches of science which do not bear so strongly and directly on public utility, as to offer by their cultivation the prospect of fair commercial returns, are left altogether to their own unassisted resources. Yet, even in the vulgar calculation of pounds, shillings and pence, it is a question, whether the policy of the French Government is not superior to ours. It is at all events certain, that their well furnished museums, with the fame of their *savans*, have the effect of attracting numerous visitors to their capital. It is equally certain, that if any one thirst after scientific knowledge he will go to Paris, as being sure to find there such aid, with ‘ all appliances to boot,’ as can be had in no other city in the world. In fact, if London be considered the capital of the commercial, Paris may be said to be that of the scientific, world, which, though a smaller body, is not without its influence ; at least for good, if not for evil. Paris is pre-eminently the city of the sciences—yet why it should be so, save from the indifference of our influential personages, it would be difficult to say.”

Captain Herbert enters still more fully into the subject in his analysis of Mr Babbage’s book, which he appears not to have seen when he composed the preceding extract.

“ The picture,” says he, “ which is here drawn of the state of English science is melancholy in the extreme, and certainly makes the wonder less that we should be so much below the nations of the continent in the cultivation of every branch of science. In one point, however, we differ from our author : he considers the vile system he has exposed, to be the cause of the decline of science : we, on the contrary, are disposed to think that science must already have been at a miserably low ebb in any country where such things ‘ can be, and not overcome us as a summer cloud with special wonder.’ Where such things could pass as every-day matters, there science must have already declined.

“ That the low state of science in England is mainly owing to the system of education, is an opinion we had ventured to give be-

fore seeing the present work ; and we are glad to see this opinion confirmed by one so much more competent to judge. He observes—‘ A young man passes from our public schools to the universities, ignorant almost of the elements of every branch of useful knowledge ; and at these latter establishments, formed originally for instructing those who are intended for the clerical profession, classical and mathematical pursuits are nearly the sole objects proposed to the student’s ambition.’ The pertinacity with which we cling to a system of education established in the dark ages, and for a particular class, is indeed characteristic of our nation—as one jealous of innovation and intolerant of change. Yet the many practical evils of our perseverance in so absurd a course, generally acknowledged as they are, would seem to be more than sufficient warrant for a new experiment. That some improvement must take place sooner or later, seems evident, but in the meantime we have lost ground in the race, and have allowed ourselves to be outstripped by all the world, even by that scion from our stock, America.

“ It is a consequence, it appears to us, of the little attention paid to science in England, that, unlike the other countries of Europe, it has no separate class or profession, devoting itself to the exclusive cultivation of science. All our scientific characters belong to other professions, on which they depend for their provision, their scientific labours being occasional and desultory. One consequence of this state of things must be, that owing to the claims on their time made by their profession, few of the investigations they engage in can be pursued to a satisfactory issue—nor is it scarcely possible that they should ever attain excellence while their attention is thus divided. A second consequence is, that there is wanting that stimulus of professional rivalry which would often prompt to great discoveries, and equally that wholesome supervision exercised by a profession over all its members, which, giving to each man the credit which he is justly entitled to, is as certain to reward merit as it is to mark deficiency. If a man, ignorant of law and lawyers, wish to employ the first counsel, let him inquire to whom the profession generally look up, and he is not likely to be disappointed in his choice. So also in other professions or distinct classes, in which an *esprit de corps* seems to give every,

even the lowest, member a sort of interest and pride in the success and efficiency of the highest. But scientific men in England have no *esprit de corps*—the consequence is, they are divided into coteries, parties, cabals. The interests of science are thus lost in the squabbles and party differences of individuals, and the result is every way mischievous. It is not the least evil of this state of things, that no union of effort, or concert of aim, can be expected from them.

“Our system of education is undoubtedly the root of the evil, yet it is not sufficient to account altogether for the low estimation into which true science has fallen, or for the fact of its having been almost universally replaced by the contemptible system of tricking and puffery here exposed. An almost equally powerful cause will be found in the want of national encouragement, which is the disgrace of England. In the true spirit of a *nation boutiquiere*, it is said that every commodity, and science amongst the rest, will always, if left to itself, fetch its true value; and that, if required by the public, the demand will necessarily occasion the requisite supply. But the maxim thus applied is as erroneous as it is contemptible. Mr Babbage has well shown that science consists of two parts, theory and practice—in other words, principles and useful results. The latter are the effect of the application of those principles to the affairs of life. Now the investigation of each class of truths is so distinct, that seldom or ever do the qualities of mind, requisite for their discovery, unite in the same individual. But it is the latter alone that the public know any thing of, and consequently to those who excel in the discovery of practical applications, and to them alone, is the encouragement of the public given. The investigator of the principles which have been applied, though equally entitled to the reward, is altogether lost sight of. The discoverer, for instance, of the principle of latent heat, as it has been called, had no share of the immense reward which fell to his lot, who suggested merely one application of so general a law. Yet had Watt not known of this law, would he have ever stumbled on the capital improvement of condensing the steam in a separate vessel. The inventor of the reflecting quadrant could have done little but for the previously known laws of optics; nor could the astronomer have

derived much benefit from the instrument, when invented as applicable to determine the longitude, but for those abstruse researches of the mathematician, which, assisted by a few observations, enables him to predict the exact place of the heavenly bodies. It will, in fact, be found throughout, that theory and practice must go on together, or, to speak more correctly, practice cannot move a step but as she leans on theory. The limits of the latter will be the limits of the former. Now what encouragement has any one, however well qualified by nature, to devote himself to this branch of the subject? If he be independent, he may perhaps, urged by the strong bias implanted by nature, pursue a path where neither profit nor distinction, as emanating from the government, await him; but if he has to struggle against the *angustas res domi*, he will of course turn his attention and talents to some more promising occupation.

“ In other countries the case is different; there men of science not only reap distinction and pecuniary reward, but are considered eligible to the first appointments in the state. Mr Babbage gives a long list of these; amongst which we may mention, as known to our readers, Laplace, a Marquis, and President of the Conservative Senate; Carnot, Minister of War; Chaptal, a Count, and Minister of the Interior; Cuvier, a Baron, and Minister of Public Instruction: the latter, too, having to struggle with the prejudice against his religion, (the reformed.) The consequence of this difference of system is, that on the continent, and specially in France, science is in the most flourishing and prosperous condition; while in England we are day after day losing all that once made our distinction. Even our mechanical arts connected with science, the encouragement of which can never be trusted to the commercial spirit, are fast losing ground. The achromatic telescopes of Dollond, which once made us a name all over the globe, have been eclipsed by the productions of an establishment at Munich, assisted as they were by the successful efforts of an obscure Swiss clock-maker, to manufacture glass of a superior quality, and in larger pieces. This is one of the many practical evils attending that total indifference to the scientific character of the country, which is particularly indicative of a Government incapable of apprecia-

ting the value of science, and ignorant of the fostering cares required to assist its progress."—Pp. 229-230.

In order that our readers may possess the opposite view of the question, we quote the following observations from the *Quarterly Review*, No. 89, which profess to be the substance of what has been urged by *eminent* ! scientific persons, in reply to Mr Babbage and his friends.

"Men of science," says the Reviewer, "continue to be much divided in opinion as to what is, and what is not, the sort of encouragement which the government of this country ought to give to scientific inquiry. The views of Mr Babbage, and many others, have been abundantly explained to our readers in a late article; but these are strenuously opposed by persons equally eminent,*—according to whom the case stands thus:

"Whenever the investigation of certain topics would be useful to the public service, but, from whatever cause, such investigation is not likely to be undertaken by individual members of the community, it is the clear duty of government, acting as stewards or agents of the public, to employ the means placed in their hands for the advancement of such objects; but when the members of the community are themselves willing and competent to prosecute the inquiries, the interference of government would be not only useless, but hurtful. There cannot, they proceed, be a doubt that the philosophical pursuits to which the learned members of the French Institute devote their time, are highly beneficial to their country; or that the government of France does well to pension the savans, and to invest them with honours, that these able men may be stimulated to fresh inquiries. If science in that country were not kept up by the protecting hand of government, it would soon languish and die;—not because those accomplished philosophers, who are the admiration of Europe, are at all lukewarm in their pursuits, but because there is not, in the community at large, either a sufficient degree of taste for such things to render them generally fashionable, or enough of wealth to make them profitable. In England, they assure us, it is quite otherwise. There is not only a very extensive taste for every description of science diffused throughout our country, but there is ample wealth, always

* Who are the persons *equally eminent* with Mr Herschel and Mr Babbage?

ready, upon the slightest hint, to support the expences of such investigations. (!) * For government, therefore, to interfere in England with such things, would be like dashing with the oar to accelerate the cataract. There is abundance of momentum already impressed upon the body, and any additional force applied to it would be wasted. It matters little what may be the object of inquiry in this country; whether it be abstract and refined, or practical and immediately useful—or whether it combine the practical with the speculative—or even if it be altogether absurd and visionary. Let any subject of inquiry be started, it is straightway pursued with ardour; and there is no instance that they have heard of in which any such inquiry has been retarded for want of due encouragement. (!) † Proceeding to examples, they tell us there was a time when the Royal Society was supposed to be the only grand fountain of knowledge in the kingdom. At length, certain of its members conceived their favourite pursuit not sufficiently attended to, and in a few months was established the Geological Society. Since that period, the astronomers have, in like manner, set up for themselves; and so far from being in want of further encouragement, these interesting bodies derive, from the pockets of their own members, every assistance they can possibly desire. These are only two out of many similar societies in London, all of them numerous, and all of them supported by the voluntary contributions of their own members. But the other day Mr Barrow pointed out the want of a society which should devote itself expressly to geographical subjects, and in less than three months, five hundred and fifty members came forward to enter their names, all ready to pay their subscriptions, and upwards of a hundred of them to compound by a ten years' purchase for their annual payments. We are asked if, after this, we can question the sincerity and zeal with which scientific objects are *de facto* encouraged in England? We are told that we should surprise our readers were we to lay before them an account of the sums of money actually paid annually by the scientific and literary bodies of the metropolis alone, to say nothing of those which may be

* Where is this wealth to be found? The statement is most incorrect.

† These persons must be very ignorant of the state of scientific inquiry in England.

found in almost every town in the empire. We are asked if the money thus raised by voluntary and cheerful contribution, and expended most judiciously on purely scientific objects, is not only fifty times greater than the whole amount so ostentatiously distributed by the government of our neighbours, but fifty times greater than the executive of this country could venture to collect for any such purpose, or would be permitted to bestow even if they did collect it? Then as to titles and other external distinctions, is it not precisely because such honours are distributed with a sparing hand in this country, that they are felt, when bestowed, as of real value by the individuals selected, and carry solid weight with the body of the nation?

“ Having thus stated the substance of what has been urged among scientific people, in reply to Mr Babbage, and part of our own reviewal of his late treatise, we consider ourselves as having given all readers the means of judging for themselves between the contending parties. Truth, as in most cases, may most probably lie between. That all Mr Babbage’s complaints and our own have been answered we do not think; but croaking is not our element, and it is a pleasing relief to do justice to the admirable style in which, with regard to one great object of scientific inquiry, the British government in our day has unquestionably done its duty—carrying the wishes of the country into effect, by bringing its resources to a focus, and applying them in a manner which the government alone could have had the means of doing,—we allude to the case of arctic discovery. * * * * *

“ It is fair, then, to own, that in our view, Mr Babbage and his allies should not have passed in silence what has been done by the executive for this single investigation.”

With some of these observations “ Mr Babbage and his allies” will not be disposed to quarrel; for they have themselves experienced how great a demand England makes upon the generosity of individuals for the support of her scientific institutions: but this very fact is the result of the indifference of the government, and is the most satisfactory proof of it that can be desired. We beg, however, to remind the learned author of the preceding extract, that societies may meet in countless numbers,—may elect their functionaries,—and publish their transactions,—and

compound for their annual payments,—and “eat their white bait,”—while inventive genius has been blighted by the disfavour of government, and all original scientific research entirely extinguished. The enormous sums of money collected by societies, and which, by a strange hyperbole, the reviewer states to be fifty times greater than that distributed by the French government, afford an irrefragable proof of the position maintained by Mr Babbage. They are wasted in house-rent, taxes, wages of servants, and expensive transactions; and it would be difficult to point out any well marked instance where the funds in question have been applied to promote the direct interests of science or of scientific men.

With regard to the Arctic expeditions, Mr Babbage and his allies had nothing whatever to do with the consideration of them; and in asserting the scientific pre-eminence of France, no notice was taken of her admirably conducted voyages of discovery. A British ship of discovery may be at work in every parallel of latitude, and yet the mathematical and physical, and even the natural knowledge, and the scientific arts of England, may be utterly extinct. The interests of naval men would indeed be promoted; but as men of science have been carefully excluded from all such expeditions, we must judge of their utility solely by their results. Of these results we have often spoken in the highest terms of praise; but if we compare them with the enormous expence at which they have been obtained, they sink into comparative insignificance; and we have no hesitation in stating, that the enormous sum admitted by the reviewer to have been lavished on our *nine voyages of discovery*, would have formed a capital for endowing the most noble establishments for encouraging and rewarding scientific merit, and promoting the best interests of England. The ministers of Great Britain never failed to distribute the national treasure with a liberal hand, provided it was to be spent by the Lords of the Admiralty, or the Board of Ordnance. Offices, and honours, and pensions, were heaped in profusion on military and naval men, as if no other kind of merit deserved their notice. But it is to be hoped that these dark times are gone,—that the eclipse of science is over,—that the arts of peace will replace those of war,—and that a regenerated government will study to foster the true interests of their country.

ART. II.—*Observations respecting Professor Leslie's Formula for the Decrease of Heat in the Atmosphere; and his Opinions respecting the Polar Temperature.* By JAMES D. FORBES, Esq. F. R. S. Ed. Communicated by the Author.

MY DEAR SIR,

As I know that in every question of literary justice, *nunquam sero* is your motto, I cannot help drawing your attention to an article which appeared some years since in your *Journal*, communicated by an anonymous correspondent. It is in No. ix. First Series, and has for title, "Demonstration of Professor Leslie's Formula for determining the decrease of Heat depending on the Altitude, without 'a delicate and patient research.'" It is only very recently that, having paid attention to this very curious subject, I have been led to examine every statement connected with it which came under my observation. Now the article to which I allude excited my particular attention, and the result of my inquiries into the merits of the case was, that the author of it must, to say the least, have been very imperfectly acquainted with what had been done on the subject. His demonstration amounts merely to this, that, from the ordinary logarithmic formula, he undertakes to give one which shall be identical with the law of uniform decrease of temperature, at the rate of 1° Cent. for 81 fathoms of altitude. To do this would indeed have been a small boon to science, and we might certainly have expected, that, instead of confining himself to the undemonstrated enunciation contained in Professor Leslie's *Elements of Geometry*, he would have consulted the article *Climate*, in the *Supplement to the Encyclopædia Britannica*, which was even then published some years, where he would have discovered, that the "delicate and patient research" alluded to was of an experimental kind, and so deserving of that appellation, that, from the very refined nature of the experiments, (which were instituted to determine the capacity of air of different degrees of density for heat,) few philosophers have been disposed to place the same confidence in the deductions to which they lead, as the Professor himself has done. Indeed, previous to the appearance of the article before us, a highly distinguished philosopher had expressed his

surprise at the coincidence of observations, with principles deduced from such experiments, which could only be repeated on the small scale.* But what makes me farther regret the appearance of the article in No. ix. of your *Journal*, is, that the demonstration from the logarithmic formula had actually been given by Mr Ivory in the paper to which I allude,—not, however, for the purpose of showing that Professor Leslie's formula was incorrect, because it deviated from the law of strict proportionality to the heights,—but to point out the relation which it bore to such a law,—a law which, I believe, is now supported in all its generality by few philosophers. Let the formula, therefore, be examined on its own merits, such as they are; but let it not be condemned as a mere rude approximation to a known law given under a new form. I may also add, that, instead of referring it to the barometer, as in the expression $26 \left(\frac{B}{\beta} - \frac{\beta}{B} \right)$, Professor Leslie now puts it under the more correct form of $25 \left(\frac{1}{\theta} - \theta \right)$ where θ is the density of the air, which includes the influence of temperature.

Having thus acquitted myself of what I considered it my duty to do when I was enabled fully to clear up the facts of the case which had recently engaged my attention, I take this opportunity of saying a word or two upon another speculation of the same philosopher, which I am afraid has less ground in experiment than the so called “delicate and patient research.”

Whilst I saw Professor Leslie's opinions on the Mean Temperature of the Pole confined to purely scientific works, I had comparatively little anxiety to attempt to show their fallacy, because the few who might there peruse them could in some measure be considered as judges of the data upon which they were founded. But when I see the same theoretical deductions brought forward in support of assertions, against which conclusive experiments strongly militate, in a work of a purely popular character, and of considerable circulation, † I could have wished to see the question brought forward in its true light by some one more competent to the task than myself;

* *Philosophical Magazine*, 1821.

† *Edin. Cabinet Library*, Polar Regions, p. 12.

but a second edition having appeared without any amelioration in this particular, I cannot help pointing out, in the *first* place, the inexpediency (to use no stronger a word) of putting conclusions, purely speculative, against the weight of stubborn facts; and, in the *next*, I shall endeavour to show the fallacy by which these two results have been set in opposition to one another.

I say nothing as to the general question of Mayer's empirical law of climate; I confine myself to the important assumption to which its principal deviations from observation may be traced, that the mean temperature of the Pole is 32° Fahr.,—an opinion which Professor Leslie long maintained, and has only now modified, by reducing it to 28° Fahr.,—the freezing point of sea-water. That in the time of Mayer such a position should have been maintained, need not much surprise us, since few observations had been made of the meteorology of the higher latitudes; but that in our own day, when Humboldt had shown that the Isothermal line of 32° , passed between Lat. 65° and 71° in Europe, and as low as 53° in America; * and above all, when the direct experiments of our Arctic navigators had shown that the mean temperature of the Pole must approach zero of Fahrenheit nearer than 32° , some strong cause must be assigned for scepticism. The grand obstacle to the admission of these truths is the supposed consequence of so low a temperature in increasing to an indefinite extent the accumulation of ice in the Arctic regions,—an effect which has certainly not taken place; for (as Mr Leslie justly remarks) the length of the day would thereby be increased, which can be shown not to have happened even to the most minute amount during the last 2000 years. Now, taking up the question even at this point, we have a right to call upon all who support the modern system of inductive philosophy, to subject speculations to facts, and, however singular might appear the stationary quantity of Polar ice, to put into the class of unresolved phenomena the low temperature of the Polar regions, determined during a large portion of several different years, by Parry, and Franklin, and Scoresby, and Lyon, and, least of all, attempt to avoid the force of the cumulative evidence of registers, which meteo-

* *Mémoires d'Arceuil*, tome iii. p. 525.

rologists at home would do well to emulate, by ascribing the monstrous deviation of *twenty-five or thirty degrees* to "latent and material inaccuracies."

But, upholding in the first place such a submission of theory to experience, I propose, with all deference, to offer an explanation of the supposed anomaly, that ice does not accumulate under the Arctic Circle. This I shall endeavour to do by showing, *first*, the difference of operation in the formation and dissolution of ice; *second*, the influence of marine,—and *lastly*, of aërial currents.

I. That the mean temperature of a climate is the only index of the preponderance of the power which forms, or that which dissolves, ice, has been thought an undeniable fact, because, though vegetable productions may, under other circumstances, be equally matured in climates differing much in mean temperature, this arises from the comparative intensity of the heat of a short summer, which is yet sufficiently long to bring particular crops to perfection. But, with regard to the summer and winter effect, however distributed, upon ice, it is argued that the effect must depend solely upon the mean temperature, because the conversion of water into ice, and the reconversion of the latter into the former, require a degree of comparative cold in the one case, and of comparative heat in the other, which admit of arithmetical estimation, and are precisely commensurate. All this is undeniable; but if we examine the mode of the formation and dissolution of ice, we shall find an admirable provision of nature for preventing the former action from overbalancing the latter. Suppose the sea reduced at the surface to 28° ; if a sudden fall of temperature in the air takes place, ice will be formed very rapidly,—even to several inches in thickness in one night. But observe the effect of the continuance of cold: the thickness of the ice cannot be increased on that side next the frigorific influence, namely, the atmosphere; the *material* is the sea-water, and exists only below; the influence of that intense cold, therefore, which lowers by so great a number of degrees the annual mean, must be exerted through the difficult conductor of the mass of ice already formed, and the intensity of its impression is always rendering more tedious the accession of cold to the sea-water, by thick-

ening the defence of ice; the mean temperature of the air may thus be indefinitely low, whilst that of the junction of ice and water cannot be under 28° . But mark the contrast of the process of thawing. Here the *material* for operation, or the ice, is always exposed to the atmospheric influence; here there is no difficultly conducting medium to shelter it from the opposite extreme; but not only this, for we have the authority of Professor Leslie himself for saying that the dissolution commences *next the water*. It is actively going on upon both sides of the ice at once; the currents of warmer water ascending from lower latitudes are able to effect the partial destruction of the fields before the solar rays have acquired sufficient power to commence their active energy above. But not merely do these causes combine to this great end, but the fall of rain, and the insinuation of moisture through the pores of the ice, tend to its rapid dismemberment; yet Mr Leslie has confined his calculations to the mere thickness of a cake of ice, which the solar rays, with a given degree of obliquity, can dissolve.

II. We must attend to the important, but little known, operations which nature is constantly performing through the agency of marine currents, before we can pronounce upon the quantity of ice melted under a given latitude or mean temperature; and we have just quoted the best authority for the fact, that the commencement of the thaw is from below. The laws of hydrostatics prepare us for the fact, that currents of various kinds, and modified by an infinity of local circumstances, must effectually prevent stagnation in the ocean, and preserve a certain interchange between the Equatorial and Polar regions. Their precise nature and extent must often be disguised or wholly unperceived, especially from our ignorance of causes in action at great depths, which must be materially modified by the remarkable law of the irregular variation of the volume of water by heat, from which it follows that water, perhaps near 50° of temperature, may have a superior specific gravity to surface water at the freezing point of the ocean, or 28° . It is easy, therefore, to speculate upon the sudden effects which may ensue from the action of currents at the critical moment of the disruption of the ice, and, though easier to speculate upon than to analyze, there are none so bold as to deny

the powers which nature thus holds in store, and which may be operating the most important ends, although out of the reach of our senses.

III. The last point upon which I have to insist, (for I only touch upon what I consider the most important facts,) is the influence of aërial currents, or the strong north winds which prevail in the Arctic regions at the time of the breaking up of the ice. If we refer to the details of navigators, we shall find that the phenomena of this period are very different from what philosophers in their closets generally set about theorizing upon. The latter would imagine to themselves a lake or pond having acquired a few inches of ice at a temperature little below 32° ; here indeed the process of the dissolution is slow enough;—but how different is ice in its own proper region, with a temperature of about 30 below zero, in the month of March; it suffers a rapid and irregular expansion of volume in the succeeding month, when winter suddenly rushes into the comparatively intense heat of the short-lived Arctic summer. The icy fields are ruptured with tremendous violence, and separate with a fearful crash from the eternal glaciers of the ice-bound shores to which they were cemented. All causes become at once ripe for action; the expanded volume of the crust,—the liquefying influence of now powerful solar rays, and the penetrating effect of showers, prepare every thing for the action of those prevailing northerly winds, which, by the accounts of navigators, in a few hours change totally the whole aspect of the horizon, as far as the eye can reach; and hurry whole thousands of acres down to more southern climates, where the greater part is speedily melted by the joint influence of solar warmth and a higher mean temperature of the ocean; but some masses even reach extremely far into the Temperate Zone. Can we then fail to see a vast difference between the process of freezing and of dissolution; and have we not shown, even in this rapid sketch, sufficient causes to account for the apparent anomaly which we proposed to explain? I perfectly agree with Mr Leslie, that, “to discuss with accuracy the question of the periodical formation and destruction of the Polar ice, it becomes necessary to explain the true principles which regulate the distribution of heat over the globe;” and I have only to regret

that he did not, as he promised, “ attempt to perform it *independent of every hypothesis*, by a direct appeal to experiment and observation.”* I remain, my Dear Sir, ever most truly yours,

JAMES D. FORBES.

5th March 1831.

To Dr BREWSTER.

ART. III.—*Observations on the Meteor called the Aurora Borealis, with Calculations, Trigonometrical and Analytical, towards determining the position and the course of the Arches, with respect to the surface of the Earth.* By R. POTTER, Esq. Junior. Communicated by the Author.

THE magnificent meteor called the *Aurora Borealis* has been one which has awakened throughout all ages the feelings of wonder and astonishment in the minds of mankind. Amongst the superstitious it has been always regarded as an omen of fearful portent, betokening the most awful calamity to the inhabitants of the earth; and though we now look at it with a philosophic eye, and speculate on its connections with electricity and magnetism, its wonderful and ever-varying forms of shining nebulous masses, of spiring evanescent streamers, or more lasting and not less magnificent arches, with the brilliant tints which it occasionally assumes, excite still in us amazement and delight whilst viewing it as an exhibition of one of nature's operations on a scale which our imagination in vain endeavours to grasp. It is no matter of surprise, then, that since the revival of learning, this meteor should have often attracted the attention of philosophers to consider its nature and causes, and, in connection with these, its position in space, or its height above the surface of the earth: indeed, until we can with some precision determine its locality, it is in vain to speculate on the extent of its dependence, either on the atmosphere or even the more ponderable parts of our planet; and hence to determine its height is the first step we can take in the investigation, and it is a matter of disgrace to science at this day that such discordant opinions should prevail amongst scientific men on the subject.

Amongst our countrymen, Mr Cavendish had, by trigonometrical measurement, computed the height of an arch to be between 52 and 71 miles, and afterwards Mr Dalton, by the same method, on a base of 22 miles, found the height of an arch to be about 150 miles ; see his *Meteorological Essays*, page 175 ; and from the observations of others, he has since, see *Phil. Trans.* for 1828, brought out the height of another arch to be at least 100 miles. These computations would undoubtedly place the meteor at a distance from the earth at which the atmosphere, if it reaches it at all, must be of the greatest rarity, and where the aurora must be almost beyond the immediate effect of the ordinary changes near the surface.

An opinion has been adopted by Lieut. Hood and Dr Richardson, from observations made during the northern overland expedition, that the meteor occurs in the region only just above the clouds, and the same view has been supported by Mr Farquharson of Aberdeenshire,—see his papers in the *Phil. Trans.* for 1829 and 1830. These observers have made no sufficient trigonometrical measurements to give even a plausibility to their views ; they seem to have been governed in their opinions by hypothetical assumptions of the aurora being an electrical phenomenon, depending on the condensation of aqueous vapour ; instead of first seeking *correctly* its height, and then forming their theory.

It is so easy a thing to any person who sees an aurora which passes nearly over his head, to convince himself, even without either instruments or possession of the ordinary mathematical knowledge, that it is more than a few miles from him ; that I consider it a reflection on any one's capacity and judgment, who, having once seen it so situated, comes to the conclusion that it is only just above the region of the clouds. And of those who have not observed the aurora, and would yet be anxious to support the same opinion, I should say unhesitatingly, that they are either ignorant of what has been done on the subject, or entertain an unphilosophical bias, which prevent their examining it correctly.

I have myself watched many *Aurora Boreales*, and had the good fortune to observe the one on the evening of the 29th September 1828, of which I left a memorandum of particulars with Mr Dalton, the next morning. Accounts of the same

arch have been given in the *Philosophical Magazine*, by Dr Burney of Gosport, Davies Gilbert, Esq., Captain Kater, and Mr Utting. And three gentlemen of my acquaintance, who, having arrived by the steam-packet, were proceeding from Kingstown to Dublin outside the coach, were struck with the brilliancy of the arch, which passed almost exactly over their heads. And it is rather an unaccountable circumstance to those who adopt Lieut. Hood's and Dr Richardson's views, that the whole of the observers speak of seeing only one arch.

To meet the objection, that the observers at different places are generally viewing different arches, I, a short time since, caught the idea, that, if we knew the direction which these arches, (the only part of the phenomena which afford sufficient data for determining the height in the first instance with any degree of exactness,) take over the surface of the earth, then when they are at different elevations, their *apparent* curvature cannot be the same for every different one; but on the assumption that we have found the law of their true curvature, their height and distance might be determined from their apparent one. On further considering the subject, I found that, if the law of the magnetic variation were exactly ascertained, and could be reduced to a general formula, then the height of an arch must be determined accurately from its apparent curvature when it was symmetrical; or at an even distance from the earth in every part; the only reason we can give for the very symmetrical appearance they frequently take; and it is universally allowed that they then are found at right angles to the magnetic meridian.

With this basis we may now examine the different theories which have been advanced, and compare them with trigonometrical computations. Lieut. Hood, Dr Richardson, and Mr Farquharson consider the whole phenomena as at low elevation; then, as small portions of the arches on that view could only be seen, and being at right angles to the magnetic meridian, they would not, in the whole of their visible extent, differ sensibly from a great circle; and Captain Kater says he traced the one of the 29th September 1828, to coincide nearly with a great circle. The problem of determining the height of the aurora from *one* observation, by considering it as an arc

of such a circle, depends on the possibility of the solution of this question from the following data: A plane being tangent to a given sphere, given the angular bearings from the point of tangence, of the points where a circle concentric with the sphere cuts the tangent plane, and also the angular elevation of the highest point of the circles to determine its radius; the most moderate knowledge of mathematics suffices to tell us that no such two circles can, on the same side of the point of tangence, appear to coincide exactly in all points. Let KAM , in Plate II. Figure 6 of last Number, be the portion of a sphere to which the plane YAX is tangent at the point A , let the centre of the sphere be the origin of the co-ordinates, let the plane of xz be in the magnetic meridian, and the plane of xy parallel to the tangent plane; then CBD , being an arc of a great circle, we have given the radius of the sphere (r), the angular bearing of the highest point of the arc BAX , which is in the plane of xz , (let the trig. tang. of this angle equal e), and the angle $DA X$ in the tangent plane, (of which let f equal the trig. tang.,) let the equation of the plane in which the great circle is, be $x = uz$, then, if we have sufficient data to find the value of u , the circle is clearly determinate, but we have in the plane of xz the following $x = uz$, $z = r + ex$, and $x^2 + z^2 = R^2$, where R is the radius of the great circle; and for the points where it cuts the tangent plane, whose equation is $z' = r$, we have $x' = ur$, $y' = f x'$, and $x'^2 + y'^2 + z'^2 = R^2$, or seven equations to determine seven unknown quantities, or eliminating x, z, x', y', z' , and R , we arrive at this equation, $u^5 - u^2 \frac{2}{e} + u \frac{e^2 + f^2}{e^2 + e^2 f^2} = \frac{2}{e + e f^2}$, or substituting for u its value $\frac{x'}{r}$ we may put it under this form, $x'^5 - x'^2 r \frac{2}{e} + x' r^2 \frac{e^2 + f^2}{e^2 + e^2 f^2} = r^5 \frac{2}{e + e f^2}$ which equations will, of course, produce three values, real or imaginary, for u and x . Now, on the afternoon of the 26th December last, the sky became very regularly covered at distances with stratus clouds, and though some had very small apparent altitude, yet the whole appeared to meet almost together in two opposite points of the horizon. If we take an

example and suppose that a cloud, or an arch of an Aurora, which was a portion of a great circle, for an elevation of 30° in its centre, extended 170° on the horizon, we find by the above formula that such an object, taking the smallest value of u which the formula gives, must be at rather more than 20 miles in height to fulfil the conditions; but the arches of the aurora are never observed to have so flattened a form as this; and when we apply the formula to actual observations, the height comes out so great as to be quite improbable. And, speaking from recollection only, I believe that two arches which I have seen had so much apparent curvature, or had so little extent on the horizon, compared with their altitude, that when I attempted calculations for them, on the supposition of their being arcs of great circles, the three values for u came out quite imaginary; and such as were impossible to be constructed geometrically. Hence this supposition must be abandoned.

From all we know of the magnetic variation at present, it is more probable that Mr Dalton's supposition, (*Meteorological Essays*, page 181,) that the arches are similar to parallels of latitude around the magnetic pole, is much nearer the truth than the former supposition. On this ground the problem is equally determinate with the other, or we may say more so, as we shall find that only one such small circle can fulfil the conditions required from the data; whereas in the other, there are, real or imaginary, three values for R . It will be easily seen by any one acquainted with analytical geometry, on referring to Figure 7, that in this case we have the following equations, $z = r + ex$, $x^2 + z^2 = R^2$ in the plane of xz and $y' = f x'$, $x'^2 + y'^2 + z'^2 = R'^2$ in the tangent plane, having $z' = r$ for its equation. And also, as we consider the arch a portion of a small circle in a plane perpendicular to the supposed magnetic axis, we have $(x - a)^2 + (z - c)^2 = R'^2$, and $(x' - a)^2 + y'^2 + (z' - c)^2 = R'^2$, taking R' , the radius of the small circle considered in its own plane, where we have nine unknown quantities, and seven equations; but if we take g equal the trig. tang. of the complement of the magnetic polar distance of the place of observation A, or of the angle, MOx , we establish the following conditions, $c = ag$ and $a^2 + c^2 + R'^2 = R^2$ and the whole of the un-

known quantities may be determined. In the course of the elimination it is convenient to put $l = 1 + f^2$, $m = 1 + e^2$, $n = 1 + g^2$, and $p = 1 + c g$, and we find the value of a for the particular case of each arch to come out this simple expression, $a = r \left(\frac{g p^2 l - g m + 2 e p}{n p^2 l - n m} \right)$, which tells us that

a has only one value for each position of the arch, or fulfilling the same data. From this value of a we easily find those for

c , x , x' and R , as we have $c = a g$, $x = \frac{a^2 + e^2 - r c}{a + e c}$ and

$x' = \frac{a^2 + e^2 - r c}{a}$. With these we easily find the height

of the arch. Taking as example $e = \text{trig. tang. } 10^\circ$, and $f = \text{trig. tang. } 40^\circ$, and making g in round numbers $= \text{trig. tang. } 50^\circ$, we bring out the height of such an arch to be 276.7 English miles.

I am myself convinced that there is a very great distance between the limits in height at which arches may exist, and this will be seen by any one who notices the different apparent curvatures which they take for the same angular elevation; but I have a very singular observation, which goes to show that the same arch may descend nearer the earth. On the evening of the 20th February 1830, about half-past 11 o'clock, I saw that there was an arch of an aurora; and, from a bright mass beneath its eastern extremity, there arose several streamers, which appeared to pass through the arch, and ascend to a considerable height above it, say to 30° altitude. The air was very cold and frosty, with a north-west wind and clear sky. It was very clear underneath the arch, though at first there were some small and very black clouds, which assisted me in determining the elevation. At half-past 11 the altitude of the under edge was very near 15° , and the breadth of the arch 5° . A little before 12 o'clock, I estimated the height again, and called it only 12° to 15° , which made me suspect that I had before called it too much. A little after 12 o'clock the arch became very distinct and uniformly bright, and its under edge along the whole part, which I could see,—namely, from about the magnetic north-eastward, was quite distinctly defined, and the light appeared to be glowing off uniformly along the whole

extent of the upper edge without the appearance of a single streamer. The altitude during its brightest and best defined period was 10° to 12° . It then became gradually fainter, and its altitude kept decreasing. At 6 to 8 minutes past 12, its altitude did not exceed 10° . It then became very diffuse in the magnetic north, and at 15 minutes past 12 it was dying away very quickly, its altitude only 5° , and still no appearance of streamers from it. Now, as they are said never to proceed from the south towards the north, but the reverse, and as this arch was in the northern part of the heavens, its decrease in apparent altitude could only arise from a real decrease in its height from the surface of the earth.

One of the most brilliant displays of the aurora we have had visible here was the one on the evening of Christmas day, (for unfortunately the sky was obscured by clouds during that of the 7th January, which was so fine a one to the inhabitants of the south of England ;) and though the moon was shining brightly, and only about four days from the full, it was by far the most brilliant and extensive that I have seen. I was told about 40' past 6 that an arch of an aurora was visible; at about 10' to 7 it was regular and well-defined, passing a little below the pole star; about 15' afterwards its under edge was higher than α Polaris, and at the western extremity it passed about 6° to the south of α Aquilæ, counted on the horizon; its eastern extremity was diffuse and not so well-defined; about 7 o'clock another arch had formed at about 15° of elevation, γ Ursæ Majoris being in its under edge; about 10' after, the next star in the tail of the Bear was in its under edge, or its altitude was about 20° , at which position it remained during the display; its under edge passed about 5° to the north of α Aquilæ counted on the horizon. The higher arch disappeared and reappeared several times; when, at its most southern position, it passed very near the zenith, Capella being in its southern edge, and the more remarkable stars in Cassiopeia being covered by it; it appeared about 6° to 7° broad, and no part of the aurora passed farther to the south than this arch, excepting, perhaps, a few streamers, which shot past its western edge towards the point to which the south pole of the dipping needle tends, so that it will form a good object

for trigonometrical computation to those who have observed it at some distance from Manchester, on the same magnetic meridian. The place at which I observed it is about $1\frac{1}{2}$ miles to the north of Manchester.

These two arches were very regular and symmetrical, and *to the sight* evidently very nearly at right angles to the magnetic meridian ; but between them there occurred a profusion of irregular bands of light, taking different fantastic forms. Often they appeared like loops, turning again on themselves ; at other times they appeared of a waved form, like a ribbon thrown down at random. These bands were generally from 4° to 5° broad. They appeared to proceed from an immense mass of nebulous misty light in the north-east, and, when they had extended along the northern horizon, they appeared to light up at their eastern end, and dissipate in streamers and glowing masses, the lighting up proceeding onwards towards the west. When dissipated, other fresh bands of different forms proceeded in the same manner from the eastern magazine, if I may so call it, and were dissipated in like manner in their turn. A succession of these phenomena occurred during two hours. At 9 o'clock the aurora was dying away very quickly. About 7 o'clock the western end of the higher arch became of a fine bright ruby red, which continued for some time. Many of the streamers near that end also were of the same tint, and some were red on their western edge, and shading into yellowish green on the eastern. Though the two arches, between which the other phenomena appeared, were symmetrical and at right angles to the magnetic meridian, yet these irregular bands seemed as if less under the same influence ; for, though the whole might pass in the direction of the regular arches, yet parts of them were seen extending in directions at all angles to the meridian.

The arch which was visible on the evening of the 12th December last was observed by John Blackwall, Esq. of Crumpsall Hall, about two miles north of Manchester ; and I am indebted to him for the following particulars :—" On the evening of the 12th December 1830, at 8^h 15' P. M. a well-defined arch of an Aurora Borealis suddenly succeeded to a splendid display of streamers, which were lighted up from east to west,

and west to east, alternately. At 20' past 8 the vertex of the arch attained an elevation of about 15° above the north-western horizon, from which it did not deviate while it continued visible. The arch was about 5° broad, and was bisected by the magnetic meridian. Although the sky continued perfectly cloudless prior to 10 o'clock, this was the only arch seen, and there was not the slightest appearance of an aurora in any other part of the heavens." This arch was observed also by Dr Burney of Gosport, to whom the world is indebted for so many valuable meteorological observations. He says, in the *Philosophical Magazine*, that at the time he estimated the altitude at 8° ; but having written to him to know if he meant this for the upper or the under edge, I am favoured by his reply, stating, that, on further consideration, he thinks 7° for the upper edge might be nearer its altitude, as he only estimated it hastily to be about 8° at half-past 8 o'clock.

Though these low arches are not the most desirable for trigonometrical computations, yet this has the advantage of having been for a long time stationary, and Gosport and Manchester are very nearly on the same magnetic meridian. Where we do not estimate nearer than whole degrees, we may disregard the effect of refraction; and, calling the chord of the arc of the meridian, between the places of observation, 193 miles, and adding $1^{\circ} 23'$ to the altitude at the former place, and subtracting the same quantity from that at the latter, on account of the effect of the curvature of the earth, we find the distance of the highest point of the arch from Gosport to have been 498 miles if its altitude were 7° , or 615 miles if 8° , and its elevation above the surface of the earth 99 miles in the former case, and 134 in the latter.

The arch which was seen on the 29th September 1828, attained at Gosport an elevation of 70° , or about 20° above α Polaris. With me it was visible south of the zenith, and was in quick motion during the whole time I saw it. It passed over β Pegasi at about 15' past 8 o'clock, and, when its northern edge was about 5° south of that star, it was become hardly perceptible. Its breadth was very nearly 5° . Hence its lowest apparent altitude may be taken at 54° , or, if Dr Burney only gave its altitude of 70° , whilst still of considerable brightness, we must allow an altitude of 60° for its southern edge at

Manchester. These being corrected for the curvature of the earth, give the real elevation as from 197 to 218 English miles.

If we examine the arch of the 12th December by the formula for a small circle around the magnetic pole,—as the eastern end was observed to cut the horizon near the Lion's neck, and α Aquilæ was in its upper edge in the western horizon,—it must have had an extent in azimuth of about 115° for its under edge, and its altitude was found for the same edge to be 10° . On these data we find the value of x to be 350.56 English miles, and that of x' to be 421.67, and $R - r$, or its height 77 miles, taking the magnetic polar distance of Manchester at 41° , which it appears to be nearly, calculating from Captain Parry's observations on the position of the Pole. Then for the upper edge we have to add the breadth of the arch, which we find to be 31 miles, and the height is $77 + 31 = 108$ miles,—a quantity agreeing as well with the trigonometrical computations as we can expect, when we see that the data are furnished only in round numbers. The arches seen on the 25th December were too diffuse on their eastern ends for noticing the stars they passed near; but, calculating from the magnetic north to their western end, I see that they had less proportional extent in azimuth to their altitude than the one calculated above, and so venture to predict, that, if sufficient observations have been made for trigonometrical computations, that aurora will be found to have been at a higher elevation than the one of the 12th December.

These confirm the former calculations of Mr Dalton and others, by assigning a great elevation to this meteor; but I consider this as a secondary consideration to that of proving, when we have correct measurements with divided instruments, the extent of conformity with small circles (or say rather cylindrical rings,) round the pole, or the extent of the deviation from them. This involves the important subject of the magnetic variation and dip, which we should leave no means of investigating unexplored.

It will be seen that we are not tied to the angles of the points where the arch cuts the horizon, but may take them at any other elevations with proper instruments; and by substituting, in place of the equation of the tangent plane, that of any other given plane passing through the place of observation, we shall

obtain a similar formula. It will be seen, that, if an arch of 10° elevation and 115° extent be 108 miles high, we must see about 850 miles of its inner edge; and a much greater length of its outer edge; and we may entertain a hope, that, being on such a scale, something important may be learned on the electro-magnetic theory of the earth, by comparing the calculated curvature with the various theories.

SMEDLEY HALL, Feb. 15th 1831.

ART. IV.—On the Respiratory Organs and Air-Bladder of certain Fishes of the Ganges. By J. TAYLOR, Esq. *

AMONG the peculiarities of internal structure in the fishes of the Ganges, the most remarkable, in a physiological point of view, are those connected with the organs of respiration and the air-bladder, in certain species of the genera *Ophiocephalus*, *Bola*, *Coius*, *Trichopodus*, *Macropteronotus*, *Pimelodus*, *Silurus*, *Myustus*, and *Clupanadon*.

The *Macropteronotus Magur*, *Coius Cobojius*, *Trichopodus Colisa*, *Ophiocephalus Gachua*, and *Silurus Singio*, present, in addition to the usual number of gills observed in osseous fishes, certain respiratory organs, which admit, it would seem from the tenacity of life possessed by these species, of a higher degree of oxygenation of the blood than is effected by means of common branchiæ.

Macropteronotus Magur. This species (the *Silurus Batrachus* of Bloch) possesses, in a deep cavity on each side of the head, two arborescent branchiæ, similar to those discovered by M. Geoffroy, in the *Silurus Anguillaris* of the Nile. These organs supply the place of the superior or cranial limbs of the two middle arches, and consist of two vascular trunks, that spread out into numerous small branches, resembling a tree destitute of foliage, or rather a corroded preparation of the

* Mr Taylor has been so kind as to transmit us a copy of this paper previous to its publication in India, and has accompanied it with a series of beautiful coloured drawings illustrative of his inquiries. We regret that these cannot be given in a periodical work.—ED.

kidney: they are composed of a smooth semitransparent substance, apparently possessing the properties of the arterial tissue, and are of a deep red colour, their external surface being extensively covered by the minute branches of the branchial artery. The aerated blood appears to be imbibed from the extremities of these branches, through innumerable *villi* in the parietes of the respiratory ramifications into their internal canals, and thence flows into the small vessels that unite to form the aorta. Cuvier assigns to these organs, the double function of affording a surface for the oxygenation of the blood, and of acting as so many hearts for propelling it into the aorta.

Coinus Cobojius, and *Trichopodus Colisa*. Each of the branchial arches in these fishes, consists only of one osseous portion or limb, and is provided with very short cartilaginous laminae; the fourth or posterior arch presenting merely a rudiment of that structure. The supernumerary organ of each side lodged in a cavity, as in the *M. Magur*, and supported upon a broad stalk of cartilage, which is fixed above, by a tendinous process, to the side of the cranium, and below, to the ends of the first and second arches, is concealed from view, by a thin membrane that extends across from the bone corresponding to the clavicle and the superior extremities of the branchial arches, to the internal side of the operculum. It consists of several broad but very thin cartilaginous plates of a convoluted figure, intimately connected at their bases, and so arranged as to present the appearance of a rosette. The branchial artery and aortic vessels are ramified upon a thin membrane, reflected from the laminae over the surface of this foliated organ, giving it a deep red colour.

Ophiocephalus Gachua. The branchial arches of this species have also very short laminae. The supernumerary organ of each side is divided into two portions supported upon two broad osseous plates, one projecting from the internal side of the articular bone of the head, and connected with the cornu of the os hyoides; the other, articulated with the anterior branchial arch, the superior limb of which is wanting. The substance composing the organ is situate upon the edge of these plates, and consists of a thick solid tissue, having a curled margin, not unlike that of the common species of alga, or seaweed. The branchial artery having given off branches to the laminae of the arches, runs for a short distance in a canal of

the osseous plate, which is connected with the anterior arch, and afterwards proceeds upon its surface, where it divides and subdivides into minute branches, distributed to the tip of the supernumerary organ: the vessels that return the aerated blood unite into one branch, which joins those proceeding from the gills to form the aorta.

Silurus Singio. Besides the eight branchial arches, there are in this fish two respiratory canals, which extend along the back from the cavity of the mouth to near the end of the tail, where they terminate in blind extremities. They are situate below the superior or dorsal portions of the lateral muscles, one on each side of the vertebræ, close to their spinous processes; and are composed of two coats, of which the internal one is smooth, vascular, and of a thin delicate texture, resembling the membrane that covers the branchial laminæ. Each of these canals is provided with a large vessel, which injection shows to be a continuation of that branch of the branchial artery sent to the posterior arch. It runs along the bottom of the canal, giving off, as it proceeds to its posterior extremity, numerous transverse branches that are minutely ramified upon its internal surface. The canals open into the mouth; but the communication between them and that cavity, is in a great measure shut up by the laminæ of the second and third arches, at the junction of their limbs: these laminæ are disposed in a semicircular figure, and are so placed, in relation to each other, as to present, when the two arches are approximated, a laminated surface of an oval figure, to the mouth of each canal. The air contained in these cavities is apparently separated from the water, by means of the laminated body: and the blood, thus oxygenated upon their internal surface, is conveyed by small vessels that descend between the transverse processes of the vertebræ, to join the aorta.*

All the species, possessing these supernumerary organs of respiration, are very tenacious of life; surviving the infliction of severe wounds, and living upon land for a much longer time than other fishes. "In China, the species of the *G. Ophioccephalus*," according to Hamilton, "are often carried alive in

* If a quill, open at both ends, be introduced through an incision, at the side of the spine, into one of these canals, bubbles of air will arise from it simultaneously with those disengaged from under the operculum.

pails of water, and slices are cut for sale as wanted, the fish selling dear while it retains life, while what remains after death is considered of little value." The *Coius Cobojius* can be kept alive without water for five or six days, and, according to the same author, is often conveyed in that state to the Calcutta market, from marshes at the distance of one hundred and fifty miles. This species, and the *Ophiocephalus Gachua*, like the *Doras Costata* or hassar, Dr Hancock's *Zoological Journal*, No. xiv., possess the power of locomotion on land to a considerable extent; and are the fishes so often met with, after a shower of rain, in fields at a distance from rivers or marshes; and hence supposed to fall from the clouds.—Hamilton's *Account of the Fishes of the Ganges*, p. 68.

The air-bladder of fishes is generally found in the abdomen, adhering to the lower surface of the spine. In the *Ophiocephalus Marulius* and *Gachua*, however, it is not confined in its situation to that cavity, but extends as far as the extremity of the tail, more than two-thirds of it being enclosed between the caudal portions of the lateral muscles. It is of a cylindrical shape, and is divided internally into unequal cavities, by a transverse septum, formed by a reflection of the internal coat; in the centre of which septum there is a small foramen surrounded by a number of short radiated fibres, apparently of a muscular structure, allowing the air to pass from one compartment to the other. The external tunic, at the anterior or abdominal extremity of the organ, is apparently of a muscular texture, and is provided with a large nerve, derived from the eighth pair; but behind it is thin and weak, the powerful lateral muscles by which it is encased supplying its want of muscularity.

The same kind of internal division exists in the air-bladder of the *Macrogathus Armatus*. In both of these fishes, the vascular body which, it is generally supposed, secretes the air of the bladder, is situate on the internal surface of the anterior portion of the organ, and consists of several small kidney-shaped glands, with minute villi diverging from them.

In the *Coius Cobojius* it extends also as far as the end of the tail. Behind the bone that forms the boundary of the abdominal cavity, it is divided longitudinally by the spines that support the anal fin, into two cavities: strong tendinous fibres

run obliquely between these spines, and fill up the space between them, leaving merely a few irregular openings of communication between the two sides above and below.

In the *Trichopolus Colisa* it differs from that of the former, in having a membranous septum in its abdominal portions; thus dividing the organ into two compartments, from one extremity of it to the other.

The air-bladder varies considerably as to figure, size, and internal structure, among the species of the genera *Pimelodus* and *Silurus*.

In the *P. Aor* it usually consists of two portions of the figure of a heart placed in a line, and united at their apices.

It is extremely small in the *P. Silondia* in proportion to the bulk of the fish. It lies close to the anterior vertebræ, is of an oval figure, and is divided into two cavities, each of which, in a fish weighing eight pounds, is not larger than a hazel nut.

The *P. Pangasius* has it composed of four or more portions, extending in a line from opposite the pectoral fins, to near the end of the tail. The first is generally oval, the second pyramidal, and the two last, which run between the caudal portions of the lateral muscles, approach to a cylindrical shape. The numerous septa on its internal surface descend from above downwards in the first portion; in the second, they run in a transverse direction; and in the posterior ones, form a number of irregular cells.

In the *Silurus Boalis* it is of the figure of a heart, divided, internally, by a longitudinal septum into two cavities, which have a free communication with each other through a semilunar opening at the anterior part of the septum.*

It communicates in all of these species with the alimentary canal, by a *ductus pneumaticus*, extending from its lower surface to the œsophagus.

The air-bladder, besides possessing the locomotive function generally ascribed to it, appears, from the connection that exists between it and a set of small bones, analogous to the *ossicula auditus*, to exert some influence on the sense of hearing. These bones correspond to the *malleus*, *incus*, and *stapes* of Mammalia, and are present, according to Professor Weber, by

* This cavity generally contains a number of worms of the *G. Fasciola*.

whom they were discovered, in all the osseous fishes, in the vicinity of the anterior or cervical vertebræ.—(Blumenbach's *Comparative Anatomy*, p. 285.) They exist in the different species of the genera *Silurus*, *Pimelodus*, and *Cyprinus*, I have had an opportunity of examining; but are apparently wanting in the genera *Ophiocephalus*, *Coius* and *Trichopodus*: and also in the *Bola Pama*, *Myxus Chetala*, and *Clupanodon Ilisha*, in which species their place is supplied by a direct connection between the two organs. The bone corresponding to the *malleus* is considerably larger than either of the other two, and is uniformly situate at the anterior extremity of the air-bladder, extending along the side of the spine to opposite the first vertebra. In the *Silurus Boalis* its anterior half is long, flat, and of a triangular shape, terminating in a blunt point, while the posterior portion is crescentic, having below a scabrous surface, to which the external coat of the air-bladder is firmly attached. By means of a short styloid process, projecting from the side of the latter portion, and received into a pit on the body of one of the vertebræ, this bone enjoys a limited degree of lateral motion, by which its apex can be made to approach or recede from the spine. The *incus*, which is of a short cylindrical shape, is placed between the apex of the *malleus*, and the bone corresponding to the *stapes*, and at a right angle with the former: and is attached to each of these bones by a short round thick tendon, in the centre of which it presents the appearance of being imbedded. The *stapes* may be described as consisting of two parts: one is a thin hollow portion, of an oval figure, somewhat resembling the bowl of a spoon, having the tendon of the *incus* attached to its convex side; the other consists of a small round knob, with a minute spicula projecting from it, and is joined by a neck to the small end of the first portion. It is situate in a large foramen in the first vertebra, and forms one of the sides of a small chamber communicating with the cavity of the cranium. This chamber lies immediately behind the cavity containing the sac and *ossicula*, or calcareous bodies of the internal ear; and within the canal of the first vertebra, its roof being composed of a strong tendinous membrane, that supports the *medulla spinalis* above. The oval portion of the *stapes*, with its concave side presented towards the interior of the chamber, is loosely connected by a membrane, to the edge of

the foramen, and admits of being projected into it, to such an extent as almost to touch its fellow of the opposite side, when both *mallei* are at the same time gently depressed with the finger, upon the *incudes*. The anterior portion of the *malleus*, and the *incus* with its tendon, as far as its attachment to the *stapes*, are inclosed in a tendinous sheath, containing a quantity of gelatinous fluid: they have no muscles attached to them, and it is probable, therefore, that the slight motion which the *malleus* enjoys, through the medium of its articulation with the vertebra, is entirely regulated by the external coat of the air-bladder.

The air-bladder of the *Pimelodus Gagora*, *Pimelodus Bagharia*, *Silurus Singio*, and *Macropteronotus Magur*, is placed behind the head, close to its articulation with the first vertebra, and in this situation is connected with the *ossicula auditus*, which are conspicuous, and present the same relative position as those of the *Silurus Boalis*.

In the *P. Gagora* there are two air-bladders lodged, one on each side, in an osseous cup attached by a narrow neck to the body of the first vertebra, close to its junction with the cranium. The mouth of each of these cups is covered over by the common integuments, which are at this part extremely thin, and adhere to the surface of the subjacent bladder, presenting, when the latter is distended with air, an external elastic tumour, of an oval figure. The two air-bladders, which have no communication with each other, or with the alimentary canal, apparently derive their supply of air from a vascular tissue, placed between the two cups where they are attached to the spine: the external coat is of a thin texture, and argentine colour, and has a layer of fine adipose substance interposed between it and the internal surface of the cup.

The *P. Bagharia* has also two air-bladders, which closely resemble the former in the argentine tendinous texture of the external coat, and in having no communication with each other, or with the alimentary canal. They are situate, one on each side of the body, in a deep groove or furrow of the consolidated transverse processes of the cervical vertebrae, and are extremely small, in proportion to the bulk of the fish; each of them, in an individual weighing ten pounds, not exceeding a large

garden pea in size : they are placed at the middle of the grooves, at about an equal distance from the common integuments (immediately behind the pectoral fins) and the vertebral column ; the space between each of them and the former being filled up with adipose substance, while that next to the spine is occupied by the *malleus*.

In the *Silurus Singio* and *Macropteronotus Magur*, the air-bladder consists of two small pyriform bags, joined at their pointed extremities by an intermediate canal. It is situated across the spine ; each of the pyriform portions being contained in a funnel-shaped case, projecting outwards from the side of the body of the first vertebra, and having its mouth covered over by the common integuments, as in the *P. Bagharia*. The case is formed of bone above, and below by a tendinous membrane that extends across the inferior surface of the first vertebra, thus protecting from pressure the intermediate canal or isthmus by which the two pyriform portions are united. A communication exists between it and the alimentary canal, by a small *ductus pneumaticus* from the intermediate canal to the *oesophagus*.

In the *Bola Pama*, *Mystus Chetala*, and *Clupanadon Ilisha*, there exists a more direct connection between the air-bladder and organ of hearing, than through the medium of the small bones above referred to.

The air-bladder of the *Bola Pama* is of a pyriform shape, terminating behind in a slender elongated process. From each side of this process an appendix arises by a minute origin, and proceeds by the side of the body of the bladder, gradually enlarging in size, until it reaches its anterior extremity, opposite to which it divides into a number of blind tortuous branches, that spread out upon the lower surface of the kidney. Two of these branches, which are considerably larger than the rest, follow a very singular course : one mounts over the bone corresponding to the clavicle, and appears externally under the thin integuments at the edge of the *apertura branchialis* ; while the other winds round the cavity in which the internal ear is lodged, and almost meets its fellow of the opposite side, at its anterior part. This latter branch, (the one connected with the organ of hearing,) is dilated at its extremity into a conical shaped bag, which is of a very thin delicate texture, and is placed in a small pit or fossa

on the external surface of the base of the cranium, having merely a thin septum, generally of a membranous nature, interposed between it and the principal *ossiculum*, or calcareous body of the internal ear.

In the *Mystus Chetala* this organ extends in length from the first vertebra to near the extremity of the tail. Anteriorly, it forms several chambers communicating with each other, and behind is divided into two parallel cavities by a longitudinal septum, which, like that of the *Trichopodus Colisa*, is partly membranous, and partly composed of the spines that sustain the anal fin. These spines are long, hollow, cylindrical bones, and are connected by the internal lining coat of the bladder, reflected on both sides over their whole extent of surface. The lateral muscles forming the walls of the cavity are very thin, and send off from their lower part small slips of muscles that are inserted into the membrane between the spines, by which means the septum can be expanded somewhat in the manner of a fan. A free communication exists between the two sides of the organ, through a number of *foramina* in the membrane connecting the spines, and anteriorly between one of the chambers and the œsophagus, by means of a round opening, wide enough to admit the point of the little finger. From the anterior part of the organ a canal proceeds upon the inferior surface of the cranium, as far as the cavity containing the internal ear, opposite to which it terminates in a blind extremity. It consists of a deep furrow on the external surface of the base of the cranium, covered below by an elongation of the external coat of the bladder, (bearing some analogy in its structure to the Eustachian tube of the higher orders of animals;) and like the corresponding part in the *Bola Pama*, is separated from the principal *ossiculum* of the internal ear, by a septum so thin and delicate that the slightest touch with a pin is sufficient to rupture it. (See Note A.)

The air-bladder of the *Clupanadon Ilisha* is long, narrow, and pointed at both extremities, as in the genus *Clupea*. From its anterior extremity two very minute ducts arise, and proceed along the inferior surface of the cervical vertebræ and posterior part of the head, to terminate in two small air-bags, situate one on each side of the cranium. These ducts, when

divided with the knife, present open mouths, scarcely large enough to admit the point of a fine bristle: they are semitransparent, and bear a striking resemblance, both in structure and appearance, to the membranous semicircular canals of the internal ear. The small bags to which they lead, are lodged in two cavities of a corresponding size in the substance of the osseous walls of the cranium, in which situation they are entirely concealed from view. Each bag may be described as consisting of two portions: the first has a horizontal position, and somewhat resembles a French bean in figure; the second is pyriform, is situate above the first portion, and is united to it at nearly a right angle. These bags have very thin *septa* interposed between them and the *ossicula* of the internal ear, and differ considerably in texture from the organ in the abdomen; their external tunic being thin, and of a silvery appearance, while that of the latter is thick, and of a dark red colour, presenting a smooth internal surface, like that of a serous membrane.

§ On the Anatomy of the Cuchia.

The *Cuchia*, an animal generally regarded as a species of eel, is entirely destitute of fins, and presents, in the structure of its organs of respiration and circulation, some remarkable peculiarities, which would lead us to place it between the class of reptiles and that of fishes. It is common throughout the south-east parts of Bengal, especially in the vicinity of D'haca, where it is generally found lurking in holes and crevices on the muddy banks of marshes and slow-running rivers. Hamilton, viewing its single external spiracle or branchial aperture below the throat as a generic character, has assigned a place to it under the genus *Unibranchapertura* of Lacepede, (the genus *Synbranchus* of Bloch,) and has given an accurate description of it in his excellent work on the fishes of the Ganges. (See Note B.)

Organs of Digestion. The alimentary canal is continued in a straight line from the mouth to the anus, and measures about three-fourths of the whole length of the body. The œsophagus is considerably longer than in the eel or fishes in general, and presents internally, longitudinal folds of a white

colour. The stomach, which is slightly dilated at its middle, is distinguished from the former by its more muscular parietes, and the red villous appearance of the internal lining coat : towards the pylorus, where it is separated from the intestine by a valve, it is contracted in diameter, and is of a white colour. The latter part of the canal exhibits upon its internal surface, short villous processes, arranged in a zigzag manner, and is divided by an annular projection of the internal coat into two portions, of which the last (or the part corresponding to the rectum,) although shorter, is considerably more capacious than the anterior or duodenal portion. The liver is of a narrow elongated figure, attached to the side of the stomach, and has several deep transverse furrows upon its inferior surface : the gall-bladder, which is of an oval shape, lies near its posterior extremity, on side next to the stomach, and sends out a long cystic duct, that opens upon the internal surface of the intestine, immediately behind the pylorus. There is a long chain of small quadrangular shaped spleens, covered by a thin reflection of the peritoneum, and joined at their bases to a blood-vessel of considerable size, which extends throughout their length by the side of the intestine.

Urinary Organs. The kidneys, as in fishes, are long narrow glands, of a dark colour, running along the side of the vertebral column, and closely attached to it, as far as the anterior extremity of the abdomen. The bladder is of an elongated shape, and receives the secretion of the kidneys by two ureters opening into it at its fundus.

Organs of Generation. Those of the male are two long ducts of a greyish colour, possessing a number of lateral tortuous branches, that adhere to the external surface of a part of the first portion of the intestine. In the female there is a single pyriform ovary, containing numerous ova, of various sizes and colours, apparently in different stages of fecundation ; and hence it may be inferred that the animal is viviparous. They terminate along with the urinary organs in a common cavity, the external outlet of which lies immediately behind the anus. *The air-bladder* is wanting.

Bones. The cranium is articulated with the first vertebra by two occipital condyles, one on each side below the *foramen*

magum, and also by a single tubercle of the body of the vertebra, as in the osseous fishes. The bones entering into the formation of the mouth resemble those of fishes. The superior maxillary one is long, slender, and of a curved figure, having its convex side presented outwards: it is joined above to the malar bone, and below to the coronoid process of the lower jaw. The latter consists of two strong pieces slightly incurvated, and firmly united at their symphysis by cartilage: each piece being formed by two bones closely wedged together. Both jaws are furnished with teeth, the largest being planted in the palatine arches above; they are fixed in alveolar cavities, are sharp pointed, and of a hook form, but do not possess any structure resembling the tubular fangs of serpents, to confirm the opinion entertained by the natives, of the venomous nature of the bite of the animal. The branchial arches are situate behind the head, at the sides of the four anterior cervical vertebræ, and possess a semicartilaginous structure, exhibiting, when held up to the light, several detached osseous pieces, of an irregular figure, imbedded in a semitransparent substance. With the exception of the first or anterior arch, which is fixed to the end of the *cornu* of the *os hyoides*, all the others are articulated below to their fellows of the opposite side. The first and second are composed, each of one portion, and are free at their superior or vertebral extremities, presenting the appearance of being suspended in the membrane of the pharynx; while the third and fourth, or two posterior ones, have very minute superior limbs, which are attached to a small pharyngeal bone, connected through the medium of a long and flat muscle, running between the membrane of the back part of the mouth and the inferior surface of the vertebræ, with the base of the cranium. All the arches are smooth on their internal surface. The bones corresponding to the scapulæ, or those which in fishes support the pectoral fins, are long, slightly curved, and united by cartilage at their inferior extremities.

Connected with deglutition, and situate below each of the latter, is a small bone, which, together with the pharyngeal bones, is covered on the side next to the gullet, with small teeth. The body of each vertebra is hollowed out posterior-

ly, forming a hemispherical cavity, in which the intervertebral fluid is contained, and is united through the medium of this substance, and also by a small triangular process sent off from each side of the cavity of the medulla spinalis, with the bone contiguous to it. The spinous process is bifurcated, or rather consists of two distinct pieces inclining in different directions; and each transverse one, of a short thick spine, grooved on its anterior aspect, with a thin plate of a triangular figure arising from it behind, and occupying the whole side of the body of the bone. The ribs, which are extremely small, are attached to the points of these processes as far as the anus. The cervical and caudal vertebræ are distinguished from the intermediate or dorsal ones: the former by the compressed flattened appearance of their bodies, the latter by possessing spinous processes both above and below.

Organs of Sense. That of smelling appears to be the most perfect. The nasal cavity of each side is tubular, and has two apertures, an anterior one at the extremity of the snout, and a posterior between the eyes: and consists also of two parts, one running forwards, the other extending backwards from the anterior opening in a duplicature of the membrane connecting the malar and maxillary bones, as far as the point of union between the latter and the coronoid process of the lower jaw. The cavities of both the sides have a free communication with each other at their anterior extremities, and may be filled with vermilion injection from one of the apertures. The eyes are very small, and are covered anteriorly by a transparent skin. The *thalami optici* lie behind the cerebrum, and send out two long slender optic nerves, which cross each other without any incorporation of substance.

Organs of Respiration and Circulation. Upon the under surface of the throat there is a semilunar aperture, divided below the integuments in the mesial line of the body into two smaller openings, each of which leads to the gills of the corresponding side. The branchial arches are connected by a strong tendinous membrane, in which are three small openings of an irregular figure for the passage of the water from the mouth, and are covered below, for about one-third of their length, by a thick muscle proceeding from the bone correspond-

ing to the scapula to be inserted into the *os hyoides*. Of all the arches, the second alone possesses laminæ for the purpose of breathing; and these consist merely of a few long fibrils attached to the middle of the arch, and occupying but a very small extent of its surface; the third supports in the place of laminæ, a thick and semitransparent tissue, which in large individuals of the species presents a fringed or denticulated appearance at its edge, while the first and fourth are bare, having only the membrane that fills up the space between the arches reflected over them. The principal organs of respiration are two small bladders, which the animal has the power of filling with air, immediately derived from the atmosphere. They are placed behind the head, one on each side of the neck, above the superior or vertebral extremities of the branchial arches, and are covered over by the common integuments, presenting externally, when distended with air, two protuberances of a round shape. On dividing the skin, and reflecting it back, each bladder is found to be partially covered, at its anterior part, by a small and extremely thin operculum, joined to the upper part of the articular bone of the head, and connected below with a *membrana branchiostega*, which is supported by six osseous rays. The posterior portion of the organ extending beyond the edge of the operculum, is confined in its situation, by a broad but thin muscular expansion, running across it in an oblique direction, from the tendinous sheath of the spinal muscles above, to the gill membrane below. These bladders, which are smooth, and highly vascular upon their internal surface, do not possess a laminated structure, such as that of the respiratory bags in the lamprey; but present, when separated from their surrounding attachments, and inflated with air, thin semitransparent membranous parietes resembling the posterior portion of the lung of serpents. Each communicates with the cavity of the mouth, by a wide semilunar opening between the *os hyoides* and the first branchial arch; its lower margin, which is thick and prominent, is composed of several muscular fibres that extend between the end of the first branchial arch and the side of the spine, constituting a species of constrictor muscle, by means of which the aperture can be contracted to so small a size, as to perform, in some degree,

the function of the glottis, and thus produce a slight hissing noise, as may occasionally be heard, when the air is forcibly expelled from the bladder. The heart is situate upon the inferior surface of the œsophagus, close to its termination at the stomach: and from the extreme proportional length of that tube, is at a much greater distance from the organs of respiration than is the case in fishes. It consists of two cavities, an auricle, and a ventricle, provided with a thin membranous valve between them. The former receives all the blood of the body by four large vessels, viz. two veins conveying it from the head and anterior parts; one from the liver; and one from the muscles of the posterior portion of the spine, the organs of generation, and the other viscera of the abdomen. The first run close to the spinal column, one on each side, and are joined by a transverse branch opposite to the twelfth vertebra: they afterwards separate, and proceed, one in a straight direction, the other across the inferior surface of the œsophagus, to terminate in the auricle. The posterior *cava*, which is the largest of all, proceeds along the inferior surface of the spine, between the kidneys, to its destination; while the *cava hepatica*, after piercing the pericardium at its posterior part, runs for some distance within the cavity before it terminates in the auricle. The ventricle is of a conical shape, of a reticulated appearance internally, and possesses two sigmoid valves at the origin of the branchial artery. The latter arises from it by an elongated bulb, of an oval figure, and measures from two and a half to three inches in length. It extends along the inferior surface of the œsophagus, and divides, opposite to the posterior branchial arches, into three branches of an equal size. Two of these branches run, one on each side, between the fourth or posterior arch and the small bone connected with deglutition, as far as the vertebral extremity of the inferior limb of the former; and being there reflected backwards, unite at an acute angle, opposite to the tenth vertebra, to form the aorta. The third branch, which is the continuation of the artery, proceeds along the under surface of the arches, and gives off, in its course, a branch to the second and third of each side; it then runs forward as far as the *os hyoides*, and is ultimately expended upon the respiratory bladders. The small branches of the

second and third arches, after supplying the laminæ of the former and the semitransparent tissue of the latter with small twigs, are also continued to the bladders; where they are minutely ramified, forming an indistinct net-work of vessels upon their internal surface. Fine vermilion injection, thrown in at the root of the branchial artery, penetrates these minute vessels distributed upon the second and third arches and the internal surface of the bladders, while at the same time it fills the aorta and its branches as far as the tail. The aerated blood is returned by small vessels, forming, at the posterior part of the bladders, two short trunks, which join the branches of the branchial artery before they are reflected backwards, and unite to constitute the aorta.

The *Cuchia*, it is obvious from this account of its anatomical structure, possesses the circulation of reptiles, and the respiration, partly of that class of animals, and partly of fishes. Of the whole volume of blood contained in the branchial artery, one-third passes through the gills and respiratory bladders, while the other two are conveyed directly from the heart to the aorta, without being exposed to the action of the air. This fluid, therefore, undergoes a partial oxygenation, presenting, as may be inferred, a dark purple colour in both divisions of the vascular system, the arteries as well as the veins. Hence the obtuseness of the external senses, and want of activity observable in the *Cuchia*. It is of a dull and languid nature, exhibiting in all its movements a degree of sluggishness that forms a striking contrast to the vivacity of the eel. A few individuals, which I kept in water for upwards of two months, during the last rainy season, were observed to lie at the bottom of the vessel in a very weak and apparently torpid state, without taking any food: and seldom moved about, except occasionally to rise to the surface for the purpose of inhaling air. The respiratory bladders, although individually of a small size, afford, in conjunction with the branchial structure of the second and third arches, a sufficient extent of surface for the oxygenation of the small portion of the blood transmitted to them. They do not approach either in configuration or texture to any known modification of branchiæ, but on the contrary have a close similitude, in both these particulars, to the posterior portion of the

lung of serpents. The power which the animal possesses of distending them with air, while on land, and the necessity it is under, of rising to the surface of the water for the same purpose, prove beyond a doubt that they perform the function of lungs; and lead us to the conclusion, therefore, that the *Cuchia* is “amphibious” in the strictest sense of the word,—forming a connecting link, as it were, between the *Ophidian* order of reptiles, and the genus *Synbranchus* of fishes.

(Note A.) In this species there is a cavity on each side of the head, corresponding to the tympanum of other animals. It is of an oval figure, and is covered externally by the common integuments, which are of a smooth shining appearance, and destitute of scales. It is found to contain a quantity of gelatinous fluid, and has a thin cartilaginous plate imbedded in the latter on the one side, and attached to the portion of skin analogous to the *membrana tympani*, by a small hook-like process on the other. It leads to a small foramen on the side of the cranium. This foramen corresponds to the *fenestra rotunda* of Mammalia; and is filled up by a thick membrane that separates the cavity of the tympanum from the *ossicula*, and the membranous semicircular canals of the internal ear.

(Note B.) This fish I found in the rivers and ponds of the south-east parts of Bengal. It is said to grow to two feet in length, and six inches in circumference: but those that I have seen were shorter and thinner in proportion to their length. Europeans eat the *Cuchia* as an eel: but the natives reject it, and imagine that its bite is fatal to cattle, although less powerful on the human kind; a supposition highly improbable. The whole form of the animal, having no vestige of a fin, resembles strongly a serpent. This fish is cylindrical, devoid of scales, and lubricated with slime. The colour above is dark green, below a dirty pale red: on every part are scattered small round black spots, and short yellowish lines. Two parallel pale lines run forward from the shoulders, and at the eyes join at an acute angle two others, coming from the sides of the chest. Parallel to the last are two others, one on each side beneath the lateral line. The head is blunt and of a moderate size, but swells out when the animal is irritated. The

snout is depressed, short and retuse, projecting a little beyond the under jaw. The nostrils are near the end of the snout, very small, and each has only one aperture. The eyes are near the upper part of the head, as is also the aperture to the gills. The lateral line forms an arch before, and then runs straight along the middle of the side. The tail ends in a sharp point, is compressed, and has sharp edges above and below.—*Hamilton on Fishes*, p. 16.

DACCA, 15th June 1830.

ART. V.—*On the Question of the Existence of the Rein-Deer, during the Twelfth Century, in Caithness.* By S. HIBBERT, M. D. F. R. S. Ed. &c. &c. Communicated by the Author.

THAT the Jarls of Orkney, in the twelfth century, were in the habit of crossing the Pentland Firth to chace the rein-deer of Caithness, has been asserted by Torfæus: “*Consueverant Comites in Catenesiam, indeq. ad montana ad venatum caprearum rangiferorumq. quotannis proficisci.*”—(*Rerum Orcadensium Historiæ*, Lib. 1. cap. xxxvj.)

Torfæus wrote at the close of the seventeenth century, and I long thought that in following very ancient Scandinavian authorities, he had either misquoted his original by the substitution of *Rein-Deer* for *Red-Deer*, (which last are still the proud objects of the chace of the Chatti,) or that, in preference to the language of the more authentic histories of the Jarls of Orkney, he was incautiously perpetuating that of the romantic Sagas. This sentiment, which I stated many years ago at a meeting of the Society of the Antiquaries of Scotland, I must now condemn, without, however, acquitting Torfæus of total inaccuracy.

The manuscript sources whence Torfæus drew the chief materials of his annals have been explained by a learned Iclander, Jonas Jonæus, by whom an abstract and Latin translation have been subsequently published. (Hafniæ, Anno 1780.) This work I lately consulted in reference to Caithness, and I was not a little surprised to find, that if Torfæus had done

precise justice to his original authority, it would have been impossible for any author of the present day to suppose that for the *rein-deer* we ought to read the *red-deer*. The passage is as follows: "That var sithr Jarla nær hvert sumar at fara yfer á Katanes oc thar upp á merkr at veida *rauddýri*edr *hréina*;" which Jonæus renders after the following manner: "Solebant Comites quavis fere æstate in Katenesum transire ibique in desertis *feras rubras et rangiferos* venari."

We thus find, that instead of the Jarls of Orkney coming over to Caithness to kill the rein-deer and the roe, (*Caprea*) as Torfæus would have us read, the animals actually enumerated were the rein-deer and the red-deer, (*rauddýri*), each being supposed to exist at the same time in the north of the Caledonian Highlands.

The date of the period when Ronald and Harold, the two Jarls of Orkney alluded to, visited Caithness to hunt, has been assigned by Jonæus to the year 1159; and as the Skalds, who have commemorated the event in their verses, were generally accurate in their description of the objects of the chase, no suspicion on this score can be rationally entertained. Their historical verses were often composed during the lives of the heroes whose feats they recorded, and sung at public feasts; and it would have been as derogatory to their Skaldship to make the Jarls of Orkney kill rein-deer in Caithness, supposing that no animals of the kind had then and there existed, as for a modern bard to celebrate a tiger hunt among the red-deer haunts of Atholl.

But the question of the degree of confidence to be attached to this curious passage of the Orkneyinga Saga, may be best assigned to the critic who has shown himself familiar with the circumstances under which the Skalds composed their historical verses, and with their mode of composition. Jonæus, in a note which he has written in his *index vocum* upon the word *Hreinns*, (animal quod vulgo rangifer vocatur,) after several illustrations of its etymology, adds, "Quod maximi momenti est, evincit locus citatus fuisse tempore isto in Scotia *Renones*. Nec proprii semper Grönlandis et Lapponibus fuerunt, etenim Islandia seculo xij, teste jure Island. Ecclesiastico Thorlaco-Kettiliano Cap. xxxi. eos habuit. Exstirpati tamen sunt incolarum

incuria, qua etiam dudum periire, multo illorum damno, alia, sed domestica animalia, sues, anseres, &c."

This is an apposite remark, the interest of which is heightened by the analogy made use of concerning the extinct reindeer of Iceland.

Unfortunately, however, for the confirmation of these remote historical allusions, we have not, I believe, upon record the account of any fossil animals of the *Rangifer* having yet been discovered among the wilds of Caithness or Sutherland. But as the extinct zoology of Britain has been hitherto imperfectly studied, the objection is far from being a formidable one. Until lately, countless bones of fossil animals have been destroyed, under the impression that they were the unfit objects of any museum whatever.

Leigh, in his *Natural History of Lancashire*, (Book 3d, p. 84,) has stated, that the horn of a rein-deer was found under a Roman altar discovered at Chester, and he has given a drawing of it. But the delineation is that of a fragment only, and its character is too indeterminate. Leigh, however, was a good naturalist, considering the time in which he lived, and we are scarcely warranted in roundly asserting that he was deceived.

ART. VI.—*On Monochromatic Light, and on the Achromatism and Spherical Aberration of Eye-pieces and Engy-scopes.* By CHARLES GORING, M. D. Communicated by the Author.

THE scheme of illuminating microscopes by monochromatic or homogeneous light, in order to render their chromatic aberration insensible, appears in theory extremely plausible; nevertheless, there are many objections to it. In the *first* place, it is evident that all sorts of monochromatic illumination whatsoever must be far less intense than the ordinary kinds, in as much as they must consist of one of the seven constituent prismatic colours abstracted from the rest; for if a beam of light is divided into seven parts, and only one of them is taken, (though it should be the brightest of the set,) it must follow that it will be far fainter than the combination of the whole.

It is well known to practical men, that the most vivid white light which can be procured, barring always that of the sun, is by no means too brilliant for opaque objects ; it can need, therefore, no argument to show, that, at all events, monochromatic light can never answer for these, however much it may be condensed ; for transparent ones with low powers and large apertures it might, were it not for some circumstances to be hereafter stated. Monochromatic light must of course always be of some colour or another, which circumstance, of course, must produce a very fallacious appearance in the vision of most objects, which will compel the observer to examine them afresh with white light, in order to rectify their apparent tints. Moreover, I myself am convinced that a *real* monochromatic light is yet to seek, and shall here state the experiments on which I found my opinion, leaving it to others better acquainted with the subject than myself to ascertain whether I am in error or not. When white light is subjected to the action of a prism, it is well known that it is dispersed or resolved into seven prismatic rays, leaving a number of colourless lines, when the experiment is made with due delicacy in the method prescribed by Fraunhofer. Now, if there is any such thing as monochromatic light, assuredly a prism ought to be able to produce it ; and, accordingly, I have chosen light so generated for my experiments.

The white light of the sun reflected by a plane mirror is able to show most of the coarser proof-lined objects in a rough manner ; indeed, I have sometimes succeeded in bringing out the straight lines on the Podura with it, and this too with an instrument which had not the power of showing them with any other species of illumination. I therefore chose this class of objects in my trials with solar light, for they are, upon the whole, exhibited better in proportion by it than any other bodies. In order, however, to obtain from the solar beam a monochromatic illumination, I made the following arrangements :—I placed a compound microscope with its body in a horizontal position, and excluded all white extraneous light from it as follows :—I attached a tube six inches long, to the stage, and at the end of it fixed a diaphragm $\frac{1}{8}$ of an inch in diameter in the prolongation of the axis of this tube ; and, at

about a foot distant from it, I hung up a piece of black silk velvet. I likewise drew a shade over the object-end of the microscope, reaching close down to the slider, so that no light should get into the body of the instrument by reflexion. By these means, when its power was equal to that of a $\frac{1}{20}$ of an inch lens, I could not see my field of view. I then introduced a prism, (the edges and other parts of which not employed to refract light were carefully blacked, that no white light might be reflected or radiated by it,) between the piece of black velvet and the tube, so as to refract the light of the sun from a mirror into the microscope. The prism was placed vertically, so that either of the seven colours it produced could be brought to bear upon the small aperture at the end of the tube, to the exclusion of the rest; the spectrum being of considerable breadth, owing to the distance of the prism from the diaphragm, which was about three feet. Now, by this apparatus, I could very easily select any one of the colours I wanted, by merely moving the microscope a little to the right or left, and illuminate the object with it. The object-glass which I made use of was a plano-convex of half an inch focus, and a quarter of an inch of aperture, which, with the white light of the sun, was just able to show the lines upon the scales of the Menelaus butterfly in a very imperfect manner; and was much disappointed and surprised to find that no amelioration in the vision was produced by using any of the prismatic colours instead of the white light. Moreover, I found that *not one of the colours produced apparent achromatism, unless it truly perforated the axis of the microscope*; any obliquity of the light instantly causing prismatic colours to appear, modified by the tint of the coloured ray used, *much in the same manner as white light would have been if merely passed through a piece of coloured glass*. Now the lined objects always require oblique light for their manifestation, and consequently, never could be shown without exhibiting other colours beside those of the primary ray employed; but other objects which agree with direct light could be shown destitute of prismatic fringes, though very confused and indistinct. I may observe, that the light of the sun treated in this way is by no means too intense for microscopic purposes. I even thought that the violet ray was not intense

enough. The blue ray, with an *achromatic object-glass*, performs pretty well on the lined objects. I like it better than any of the rest, (for it most resembles day-light,) though each is capable of exhibiting the lines, *if not procured in a state of too great intensity*, by being taken too close to the prism.

I repeated these experiments with the flame of a wax taper in a dark room in the following manner:—I used the prism as before, and adjusted the wax light to a proper height to suit it, I then carefully shaded the candle, so that none of its direct light should get into the microscope, and threw the spectrum formed by the prism into the body of the instrument, taking care to use it at such a distance from the prism that the light of the taper should be fairly dispersed. I did not use the long tube in these experiments, thinking it of no use, as the room was dark, and I could not see my field of view until the spectrum of the prism was directed into it. The light afforded by the prism was too faint for the power I used with the solar light. I was therefore obliged to reduce it one-half. Even then the blue ray had not sufficient power; the yellow one had, and seemed little different in its effect from the taper itself in the same state of intensity. Suffice it to say, that I got the same results as before,—that is, when I obtained the light of the prism perfectly *direct* I obtained *achromatism*; when it *came obliquely*, the *coloured fringes instantly appeared*, modified by the tint of the ray, and much in the same way as if I had looked through a coloured eye-glass at the image produced by common light. Wishing to sift this phenomenon to the bottom, I then used the light of the taper in its natural state, and found that *there is one particular position in which it may be placed behind the stage, so as to give apparent achromatism with any sort of microscope whatever, however chromatic it may be*. This appears to be as nearly as possible in the axis of the microscope; if it is placed ever so little out of it, the coloured fringes instantly show themselves. I am confident that I could make an unexperienced person believe in the achromatism of any microscope whatever by making this arrangement;* and a most

* Upon a careful re-examination, I do not think myself justified in saying that achromatism can really be so produced; it should rather be termed

fruitful source of deception it must prove as to the achromatism of these instruments!

It is no wonder that we should have so many kinds of monochromatic lamps and light, when a common candle will answer just as well. As prisms and lenses have a great analogy to each other in their operation, I then experimented on the dispersed solar light with them, and it always appeared to me that when I could succeed in getting the light from the diaphragm thrown perfectly direct through a prism, no sensible secondary dispersion was produced; but when it came obliquely, the aperture was completely fringed by prismatic tints in their natural order. As it seemed to me that a piece of rubbed glass placed over the diaphragm caused the light to be equally diffused over the interior of the tube, whether it came obliquely or not, I tried the effect of the prism upon it, but the light always seemed to be dispersed in this way as before. (For these experiments I used the apparatus first described, only I took out the object-glass and eye-glasses of the microscope, and looked at the diaphragm through a small prism held in my hands at the ocular end of the body.) In order to have made these latter experiments properly, I should have required a heliostat moved by clock-work, for the motion of the earth subverted the direction of the solar beam. Immediately I had got it truly in the axis of the body of the microscope; and the power I possessed of counteracting this was only by means of adapting the prism to the motion of the light with my hands.* I should have liked to have dispersed the colour a second time, and fairly produced a spectrum of it on paper under the circumstances

a *great reduction of dispersion*, the effect of which is assisted by the fog occasioned by spherical aberration, as well as by the confusion produced by solar light, when I used it as before stated. If there really is achromatism, it is confined to a small portion of the centre of the field of view, and even there to a point, which seems to depend much upon the position of the eye; however, I have no doubt that it would be considered such by those whose eyes have not been sharpened by habits of looking into the defects of optical instruments.

* When the light came quite direct, I of course caught the image of the sun, which dazzled my eyes too much to permit me to be quite certain of the tints or colour I saw. It is very possible that here again there might be only a great reduction of dispersion as in former instances.

of direct and oblique incidence. I hope some person, better able than myself to look into these things, and better furnished with apparatus, will examine this subject in a proper manner. The impression produced in my mind was, that *a certain quantity of white light enters into the constitution of each of the seven coloured rays*, as water does into the constitution of alcohol, and may be rendered manifest by the dispersive power of prisms and lenses under the circumstances I have described.

At all events, *I think no effective monochromatic light is likely to be obtained for illuminating chromatic microscopes, so to render them equivalent in their action to achromatic ones.*

I shall now proceed to notice some assumptions respecting the achromatism of erecting eye-pieces and engscopes, which consist in supposing that, by arranging a system of glasses with certain intervals, we may form an achromatic combination *without correcting what I shall call the objective part separately*. Had these proceeded from more obscure and less influential persons, I should have allowed them to rest in peace; but the power of proficients in the exact sciences is so great, either in retarding or advancing the 'perfection of optical instruments, that I feel it absolutely necessary for my own honour, and that of the improvements lately introduced into use, to endeavour to rectify what seems to me retrogradations and abuses. The dicta of mathematicians of high reputation are generally received without examination, because it is conceived, and not without justice, that they can hardly say or do any thing wrong. If they could, many there are who will think it more creditable to be in the wrong with them than in the right with mere practical men. Yet optics is both an experimental and a demonstrative science; and though experiment ought certainly never to dictate to analysis, (which would be placing the cart before the horse with a vengeance,) yet the testimony of those organs which are to use optical instruments must not be wholly rejected.

A telescope and what is usually termed a compound microscope pass into each other by insensible degrees, so that it is somewhat difficult to define the one distinctly from the other,

otherwise than by drawing a line of demarcation between them, by reference to the length of the anterior and posterior conjugate focus of the object-glass. Thus we may say that a telescope has its anterior conjugate focus, or the distance of the object from the object-glass longer than the posterior, or that next the eye-piece. In an engyscope the case is reversed.* The line of separation is therefore where the two foci are equal; such being the case, we may make an object-glass of short focus, say four inches, gradually pass through all the gradations from a telescope to a compound microscope, without making the least change in the principle on which the image is formed. Now, no optician has ever yet dreamed of making an achromatic telescope by arranging a system of glasses at particular intervals, though, if a compound achromatic microscope or engyscope can be so made, we ought surely to be able to construct a telescope of some sort or other on the same principle, the two instruments, as before stated, being so similar in their action of forming an image. It is a consideration also worthy of being attended to, that the first practical opticians in Europe have now all adopted the method of making achromatic engyscopes precisely on the principle of telescopes,—that is, by correcting the objective part *by means of concaves of flint-glass*. Such men as Fraunhofer, Utzneider, Amici, Selligue, Chevalier, Dollond, Tulley, would scarcely have all adopted so difficult a task as that of making these minute achromatic glasses, if it had been possible to obtain a perfect correction of chromatic and spherical aberration without them. They are all in the habit of making *so called achromatic*, erecting eye-pieces for spy-glasses, (which are in fact nothing but compound microscopes;) and it is hardly credible, that they should not have perceived the application of them as such, if they were, *bona fide*, achromatic. But the fact is, these artists all know the fallacy of these things, and how, and under

* Perhaps a definition of an engyscope, as distinct from a telescope, may be given thus:—It is a *telescope made on a very small scale, having its object-glass and the conjugate foci thereof reversed*. It may not be generally known that an engyscope will act with a concave eye-glass, just as a Galilean telescope does, and that its focus can be adjusted by moving the eye-glass alone, instead of the whole body, as is usually practised.

what circumstances, they are achromatic. I propose to show that an engyscope cannot be made achromatic in any other manner than as a telescope is, and that the new achromatics are consequently not superfluous.

The angle of aperture of the erecting eye-pieces of spy-glasses is usually very small, not exceeding five or six degrees; the chromatic aberration, therefore, of their objective part, is nearly insensible to vulgar eyes. Moreover, it is well known that all working opticians are in the habit of over-correcting the colour in the object-glasses of those telescopes which are expressly made for viewing terrestrial objects. It is supposed by some that the predominance of the blue and purple rays, which is occasioned by this over-correction, tends to exhibit terrestrial objects better than they would be seen if the instruments were perfectly achromatic,—an absurdity which I shall not take the trouble of refuting.

The real manner in which a beneficial operation is produced by this over-correction for *colour*, is by its effect on the eye-piece, which it greatly ameliorates and improves, by neutralizing the red and orange light, which would otherwise greatly preponderate in it, so that the totality of the instrument may be considered sufficiently achromatic for practical purposes; nevertheless, under certain circumstances, the uncorrected colour of the eye-piece renders itself very manifest; as, for example, when we view some dark object, such as the rigging of a vessel or a weathercock, against the light of a clear sky. For by placing the object at the edge of the field of view, while we look obliquely at it from the opposite side, so as to catch an eccentric pencil, the uncorrected colour is instantly felt.

Now, as I am contending for the *principle* of achromatism, I shall proceed to show, that all erecting eye-pieces of the ordinary construction, though they may, under certain circumstances, and in a practical point of view, pass for achromatic, yet have in good earnest no achromaticity about them; and of this I think any man may convince himself by the following experiment:—There is a small circular disk of brass with a hole in it, placed between the two bottom glasses of all erecting eye-pieces, which, when the telescope is in action, serves only to exclude the false light which is reflected by the long

tube. The aperture of the hole in it is usually about one-tenth of an inch in diameter, or just what will allow the pencil of light from the object-glass to clear it. The telescope will be just as achromatic without it as with it, when the eye-piece is removed from the telescope and tried by itself.

As a compound microscope, the said diaphragm acts a very important part, and in fact reduces the angle of aperture of the eye-piece nearly to that of the object-glass of the telescope along with which it acted, being, as I said before, perhaps six degrees. Now it is no wonder that with so small an opening, (though unassisted by the over-corrected object-glass,) the eye-piece fixed should still pass for achromatic with those who know not how to look into its defects,—just as we see mulattoes and people of colour who have not *much* black blood in their veins pass for whites. Let, however, this said stop be knocked out, or, what will amount to the same thing, let it be opened out with a broach, *until it will admit the whole of the pencil of a perfectly achromatic object-glass having a large angular aperture*; for example, one belonging to an opera-glass, or a perspective, or some of those dumpy achromatics which Ramsden was in the habit of making, this will probably cause an enlargement of the perforation of the said stop from $\frac{1}{10}$ to $\frac{5}{8}$ of an inch, if the focus of the first glass is two inches. The ratio between the diameter of the visual pencil, and that of the object-glass will truly represent the magnifying power of the telescope, and it may be known to have attained its due aperture when none of that of the object-glass is cut by it. On trying any telescope thus mounted, it will soon be seen whether the eye-piece is achromatic or not. Or it may be tried as a compound microscope by itself, and I can only say that a man who will not be able to perceive colour in it, must be either utterly blinded by preconceived opinions, or by a complete incapacity to discuss the merits of optical instruments. Nevertheless, there is a construction for an erecting eye-piece which is *bonâ fide* achromatic to any angle of aperture as it appears to me, namely, a simple reduplication of the Huygenian eye-piece, that is using one to erect and form the secondary image, and another to view it, which will give a compensation to each part; but this construction is never employed,—at least I never

met with it, because, I presume, if we use a Huygenian eye-piece to form a secondary image, the component lenses will be nearly of the worst figure, and in the worst position, for giving distinctness, which is a quality to be preferred even to achromatism. I need not observe that such a construction could never be used by itself as an engyscope or compound microscope, because its focus being negative, or between the glasses, it can only be made to operate on an image already formed.*

In the year 1815, being then a student in the University of Edinburgh, I began my career as a reformer of microscopes, from reading the article Telescope in the *Encyclopædia Britannica*, (written by Professor Robison,) and chiefly from the consideration of a passage in it, which I shall here quote.

“ We have examined trigonometrically the progress of a red and a violet ray through many eye-pieces of Dollond’s and Ramsden’s best telescopes ; and we have found in all of them that the colours are united on or very near the field-glass ; so that we presume that a theory somewhat analogous to ours has directed the ingenious inventors. We meet with many made by other artists, and even some of theirs, where a considerable degree of colour remains, sometimes in the natural order, and often in the contrary order. This must happen in the hands of mere imitators, ignorant of principle. We presume that we have now made this principle sufficiently plain. Fig. 20, No. 2, represents the eye-piece of a very fine spy-glass by Mr Ramsden ; the focal length of its object-glass is $8\frac{1}{2}$ inches, with $1\frac{1}{10}$ th of aperture, $2^{\circ} 05'$ of visible field, and 15,4 magnifying power. The distances and focal lengths are of their proper dimensions, but the apertures are a-half larger, that the progress of a lateral pencil might be more distinctly drawn. The dimensions are as follow :—

Foc. lengths $Aa = 0,775$ $Bb = 1,025$ $Cc = 1,01$ $Dd = 0,79$.

Distances $AB = 1,18$ $BC = 1,83$ $CD = 1,105$.

“ *It is perfectly achromatic*, and the colours are united, not

* Such an eye-piece is very foggy when acting with a large opening, though it must be confessed that the focuses and the position of lenses become of little consequence in a practical point of view, if *their acting aperture is exceedingly small*, for the eye cannot, under such circumstances, appreciate their effect.

precisely, at the lens Cg, but about $\frac{1}{20}$ th of an inch nearer the eye-glass.

“ It is obvious that this combination of glasses may be used as a microscope; for if, instead of the image formed by the object-glass at FG, we substitute a small object, illuminated from behind, as in compound microscopes; and if we draw the eye-piece a very small way from this object, the pencils of parallel rays emergent from the eye-glass D, will become convergent to very distant points, *and will there form an inverted and enlarged picture of the object, which may be viewed by a Huygenian eye-piece*; and we may thus get high magnifying powers without using very deep glasses. We tried the eye-piece, of which we have given the dimensions, in this way, and found that it might be made to magnify 180 times with very great distinctness. When used as the magnifier of a solar microscope, it infinitely surpasses every thing we have ever seen. The picture formed by a solar microscope is generally so indistinct, that it is fit only for amusing ladies; but with this magnifier it seemed perfectly sharp. *We therefore recommend this to the artists as a valuable article of their trade.*”

I caused that distinguished artist, Mr Adie, of Edinburgh, to execute for me a microscope similar to that recommended by the Professor, on which no expence was spared; it of course gave an erect image. I was not content with it, its construction being too complicated, and its length inconvenient; moreover, though its angle of aperture was no greater than that usually given to eye-pieces, its chromatic aberration forced itself occasionally on my unwilling conviction in a manner which I could not resist in spite of my faith in analysis. I naturally, therefore, imputed this defect to Mr Adie's imperfect execution, and set him to work afresh to make another microscope on the plan of an erecting eye-piece of the ordinary construction, gaining the different powers by the application of Huygenian eye-pieces to the image formed by the two bottom glasses. I was very much pleased by the distinctness of this microscope, and so were all who examined it. Still, however, *it was not achromatic*, so I set Mr Adie down, *at the time*, for a bungler who could not adjust the foci and intervals of the glasses in a proper manner; and determined to have

the thing done effectually, if possible, by London artists, which resolution I executed afterwards. In the course of these experiments I found the utility of getting the power at the objective, instead of the ocular, end of the instrument, as a much sharper image was thereby obtained. I accordingly deepened, what I shall call the object-glasses, as far as one-fourth of an inch focus, and obtained a very superior microscope; an account of which has already been laid before the public.

As to achromatism, however, I regret to state that the London workmen succeeded no better than Mr Adie, though I am not quite certain if a *feeble* compensation was not produced sometimes, when the curves of the two plano-convex lenses, which composed the object-glass, were turned towards each other in the manner they have been engraved. I moreover once thought that I had got a combination that was achromatic, at least upon opaque objects; and being determined to leave nothing to be done by any man who should take up the subject after me, I resolved to apply to Mr Tulley of Islington, to have his opinion and advice on the subject. He soon convinced me of *the impossibility of obtaining achromatism in any case where an image was to be formed* from a real object, otherwise than by the action of concaves of flint-glass, and that my ignorance of the art of seeing the flaws and imperfections of optical instruments was the sole cause of my supposing I had ever obtained even an approximation to achromatism.

Now, as my respect for *truly exact* science is not surpassed by that of any man, I must confess that all these circumstances created in my mind some doubts and misgivings as to the accuracy of Professor Robison's statements and theories, not very much unlike what I have observed in farmers in the country, when unable to reconcile the actual state of the weather with the predictions of Moore's *Almanack*, in which their faith is altogether inflexible.

These theories of Professor Robison seem to have been resuscitated by Professor Airy and Mr Coddington. The work of the latter gentleman on the refraction and reflection of light, contains the substance of Professor Airy's papers on these subjects; and whether it is considered as a work of originality, or

as a compilation from the writings of our first opticians, it is admitted to be the best publication of the kind at present extant, and wants no feeble testimony of mine in its favour.

I presume that the most staunch advocate for the dignity of exact science, while he will insist on the vast superiority of its evidence over that afforded by the senses, will never contend that it is to run diametrically opposite to their testimony. I shall venture, though well aware of what I am doing, to impugn its infallibility in the case of these theories of achromatism of Professor Robison, Professor Airy, and Mr Coddington, on the ground that no artist is able to make an achromatic instrument according to them.*

All that it is in my power to do will be to oppose theories, by what we call, in common language, facts; for I must state that, when I view the subject theoretically, I am utterly unable to detect the least flaw in the aforesaid theory. It seems impossible to deny that instruments constructed according to them ought at least to be achromatic. It is not to be expected, indeed, that a poor sciolist like myself should be able to school men of such highly mathematical acquirements as the aforesaid savans: the task must evidently be left to mathematicians whose calibre is still larger than theirs. I have already partially expressed my own views concerning achromatism, and I shall endeavour now to expand them into the following propositions, which rest only on the basis of the evidence of the eyes or experience.

1. When achromatism is obtained by the adjustment of lenses to particular intervals, as in the case of the Huygenian eye-

* We surely have a right to form a certain estimate of the soundness of a theory by its fruits. I once had the honour of looking through a telescope of the Gregorian form, but constructed with mirrors of glass by Mr Tulley, according to the theory of Professor Airy. Mr T. had expended the whole of his skill upon the instrument, and had worked the said mirrors over and over again a great number of times till his patience was exhausted, yet the confusion in the vision produced was so great, that it was but just possible to recognize objects. I think when such an artist as Mr T. cannot bring a telescope something near to distinct vision, when working to the utmost of his ability by the theory laid down for him, there must be something wrong about it.

piece such achromatism is *absolute and perfect*, and not like that effected by the combination of a concave lens of flint-glass with a convex of plate or crown-glass, which never effects a complete neutralization of the chromatic aberration, as is well known. Therefore, if engyscopes and telescopes could be made by the adjustment of a system of lenses to particular intervals, their achromatism ought to be decidedly superior to that obtained by the action of concaves of flint-glass, and equal to that of a reflecting instrument.

2. The only kind of achromatisation produced by convex lenses which is known in practice, is when two are adjusted to an interval equal to one-half the sum of their focal distances, or thereabouts. (These conditions are not rigorously necessary, as the lenses may be placed somewhat nearer than that.) Moreover, the intervals seem to differ a little according as the eye-piece is adapted to an engyscope or to a telescope,—at least if the power of the former is low and the tube short.

3. Many modifications of this combination may be made, as by doubling or tripling what are usually termed the eye and field-glasses, so that the compensation for a double or triple eye-glass may be thrown upon a double or triple field-glass, and that there may or may not be an interval between the lenses composing the eye and field-glasses; still, however, the mode of operation is the same.

4. In order to form a truly achromatic erecting eye-piece or engyscope, there must be a compensation both in that part which erects and forms the image, and in that which views it; therefore *no achromatic erecting eye-piece or engyscope can be made with so few as three lenses*, because either the objective or ocular part must inevitably be without compensation.

5. An erecting eye-piece can only be made really achromatic, (if we do not employ concaves of flint-glass,) by combining two Huygenian eye-pieces, or some modification thereof, together, in such a way that one shall erect and form the image, while the other is made to view it. As to the interval between them, or what may be termed the second and third glasses, if the eye-piece consists of four, it may be greatly varied without sensibly affecting the achromatism.

6. Such an eye-piece could only be used for viewing an image, and could never be employed as an engyscope, because

it would have no external focus in front of the bottom glass ; therefore, an achromatic engyscope can only have a double compensation in the same way as a telescope has it, of which latter instrument it is nearly a modification ; and as it is exceedingly difficult to procure perfect flint-glass lenses of large diameters for the object-glasses of telescopes, it is devoutly to be wished, that, by the omnipotence of analysis, we may at length be enabled to construct them absolutely achromatic, of one kind of glass only, by adjusting a system of lenses to particular intervals ; while, with proper attention to the forms of the lenses, their spherical aberration *ought* at least to be reduced to insensible quantities, (using only a small angle of aperture,) though it could not perhaps be wholly removed.

Now, on comparing these propositions with those contained in Mr Coddington's work, under the head of "achromatism," page 262 to 269, it will be seen how completely I have been compelled to leave him on the left hand. It will be a memorable instance of the fallibility of analysis, and perhaps the first on record if the said theories are found to be at fault. I shall leave it to others, however, to draw conclusions and weigh probabilities against each other. I have always myself found that good sound theories will produce instruments which satisfy the eye ; and though it must be admitted that this organ is exceedingly coarse and easily pleased in the generality of mankind, it is nevertheless in many instances capable of acquiring a refined and accurate taste in optical instruments by a good education, just as it does in pictures and other works of art. In the second volume of Mr Hume's essays, section 12, entitled, "*of the academical or sceptical philosophy*," part second, there are some curious reasonings on the subject of mathematical demonstration, which the reader would do well to consult.

I may observe, that Mr Coddington has been at the pains of falsifying his own theories, *practically at least*, to the best of his abilities, by presenting to opticians a compound microscope, termed achromatic, which is constructed according to them. It is described in his treatise on the "*Eye and Optical Instruments*," pages 60 and 61, under the head of ACHROMATIC OBJECT-GLASS, and is represented in plate 13, figure 190. He has implicated articles 205 and 210 with the expressions in pages 261 and 263, in the treatise on the "*Refraction and*

Reflexion of Light," in the construction of this instrument; and if he had succeeded in making it achromatic, there would be good reason for asserting *that it is unnecessary to correct the dispersion of its object-glass separately*, as well as "that the intellectual and manual labour, which has of late years been expended on the construction of achromatic object-glasses, has not only been unprofitably directed," but in a manner wholly thrown away, since, if his theories were accurate, a much better and purer achromatism ought to be obtained by the simple adjustment of a system of lenses to proper intervals. I assert point blank, that his instrument is as complete a failure as any thing of the sort I ever attempted myself. I have examined one of these instruments of the latest and most improved construction, made by Mr Carey, (which, however, differed not materially from that described in the treatise on *Optical Instruments*, except in having a double field-glass,)* and can, I think, be positive that both *the chromatic and spherical aberration of the objective part was wholly untouched*, and that the *eye-piece*, consisting of four glasses, *was achromatic*. Nothing can surpass the beauty of the field of this microscope; but if I may be allowed the expression, notwithstanding its extent and flatness, nothing more can be made to grow in it than in that of any ordinary compound microscope having a well figured object-glass of the same power, and angular aperture, used with an Huygenian eye-piece, also of equal power with that applied to the instrument in question.

The dispersive and spherical error of small lenses is undoubtedly small, as I have frequently remarked; but it is not insensible even with a very small aperture to those who know how and where to look for it. Some persons use it at the first glance, others only if it is pointed out to them, and some not at all, if it happens to contradict their theories and preconceived opinions; for there is no blindness greater than that generated, not in the organ of sight itself, but in the mind which receives its impressions. We are indeed so accustomed to tolerate aberration in microscopes that it frequently is unable to stimulate our senses. I shall here instruct my reader, if I can,

* This was similar to that described in No 5. Art. xvii, p. 155 of this *Journal*.

in the way of examining the dispersion of a lens; it is as follows:—Place a small globule of quicksilver on a perfectly black ground of some kind or other without polish; let it be placed on the stage of a microscope, which must be presented towards a window in such a manner that the uncondensed light thereof shall fall freely upon the globule, for the light of a candle may be employed for the same purpose;* but it must not be condensed by a lens, or mirror, or silver cap, of which latter the globule would form an image. The object is to allow the globule to form a minute picture, either of the candle or of the window, by its own natural action. All we have to do is then to bring it into the focus of the object-glass or lens, whose dispersion we wish to examine, and to observe the phenomena presented by it, both in and out of focus. If it has dispersion, the different prismatic colours will appear according to the order of their refrangibility. I shall give a sample of those visible with a globular object-glass, of half an inch focus, and 2-10ths of an inch of aperture,† (being what would be commonly used for a working instrument,) employed with a perfectly achromatic eye-piece, and the light of a window.

In focus there is a central image of the window, which is surrounded by a disc or halo of green, which is again surrounded by a circle of indigo, the whole being contained within a border of purple.

Within the focus are found the following colours: in the

* The light of the sun may also be employed, which, when allowed to fall freely on the globule, gives a miniature image of the sun, forming a *true artificial star*, (the queen of test-objects to the initiated optician,) as the light it affords is more intense than any other, so it exhibits dispersion in the most vivid and brilliant manner.

† This object-glass was made by Mr W. Tulley, who presented it to me several years before Mr Coddington had introduced the subject to the notice of the public. † It was made by Mr T. in order to procure a pencil of light in the same state from every part of the object, and to render the field of view alike in every part which it no doubt effects. As, however, its aberration is very violent, *so as to require a strong concave to correct it*, which, when applied, utterly subverts the equalization of its various pencils, it has no advantage over an equiconvex lens; he therefore abandoned it. I believe he will not, therefore, be very jealous about the honour of paternity in this matter. A good thing has generally half a dozen fathers to it, and in as many different countries a bad thing only one.

centre of the globule is a round spot of purple surrounded by a halo of blue, a second of green, a third of yellow, a fourth of orange, and a fifth of red.

Without or beyond the focus, a disc of faint greenish yellow, surrounded by a halo of strong green, without which is a halo of blue, and lastly, one of purple.

Moreover, when an instrument is perfectly achromatic, there is a total absence of all minute fringes of colour. When it is used on transparent objects with oblique candle-light, nothing, in short, but the natural colour of the object can be made to appear under any circumstances whatever; for the secondary spectrum is too faint to be seen, unless perhaps in object-glasses of at least an inch aperture. If I had employed for these experiments an object-glass of an inch focus, and 4-10ths of an inch aperture, the colours enumerated would have been much more intense and striking; and if one only of 1-8th of an inch focus, and an equal angle of aperture, far fainter, being nearly in the ratio of the quantity of light dispersed by the object-glass.

A curious effect is produced by using an object-glass with a very large angular aperture; for example, by employing the naked opening of the aforesaid globule, which is half an inch, so strong a fog is occasioned by its spherical aberration, as to drown and confuse the chromatic, so that it appears in a great measure obliterated, or very much reduced in quantity, (though there is no doubt that it must be increased.) Chromatic aberration is never so sensible as when uncombined with spherical, as in Mr Herschel's compound aplanatic lens of one kind of glass, which, having no spherical aberration with a very considerable aperture, is so clear as to allow the chromatic to be seen in the highest perfection.*

Now it is scarcely necessary for me to ask, whether it is right or not to correct the dispersion I have pointed out; *but how is it to be done by an achromatic eye-piece?* If a man chooses to tell me that those little rainbows and fringes of colour, which appear in objects viewed by achromatic instru-

* Though the dispersion of a diamond or sapphire lens, especially the latter, is much less than that of glass, lenses formed of these gems always seem to me to colour more than glass, which is accounted for by their greater distinctness.

ments, are, in his opinion, highly ornamental, and not at all injurious to vision, I can only say I wish him a better taste. I have seen people in the streets paying their pence very cheerfully to a peripatetic astronomer for the sight of a beautiful rainbow about the moon, which he had discovered with the curious telescope he was exhibiting, and which I had certainly the honour of seeing myself, together with the beautifully prismatic tails of some aquatic insects shown by a microscope constructed on the same principle as the telescope. If people like to eat dirt they shall meet with no opposition from me,—much good may it do them.

I have certainly myself seen a thirty inch telescope of two inches and a half aperture in which there was an immense quantity of uncorrected colour, nevertheless able to show the star epsilon Bootis very well, and other things in proportion, owing to its spherical aberration being very exactly corrected.

The only case, I think, in which colour can be said to be corrected by an eye-piece, is when an object-glass over-corrected for colour is made to act with a chromatic eye-piece, as I have indeed before stated, having once caused the object-glass of an engyscope to be over-corrected, because it was to act with an erecting eye-piece of the ordinary construction. The beneficial effect of this arrangement was very sensible, and it might either be said that the eye-piece corrected the object-glass, or the object-glass the eye-piece. I am of opinion that the chromatic aberration of simple microscopes and doublets is not sufficient to produce any sensible detriment to vision, unless with exceedingly minute and delicate objects, beyond that of giving a false colouring, especially if oblique light is used. This, however, we soon learn to allow for ; it is moreover considerably concealed by their spherical aberrations.

As to the spherical aberration of object-glasses and metals, inasmuch as it is sensible to vision, I am persuaded from reiterated experiments, that the power which an eye-piece possesses of modifying or altering it is exceedingly feeble. A case, however, in which I can be positive that it does actually correct it, is in that of the Gregorian telescope, in which, if the large metal is nearly right in point of figure, for example, a little too much inclined to be spherical or parabolical, the error may be

corrected by giving the small one an hyperbolical figure. Moreover, in the Cassagrainian telescope, it is well known that the aberration of the two metals (supposing them to be both spherical) is equal to the difference between the aberration of the convex and the concave, which is a clear proof that an eye-piece may correct to a certain extent ; (for I consider that the small metals and eye-glasses of the Gregorian and Cassagrainian telescopes, in their combined action, are neither more nor less than eye-pieces, which operate by *forming a secondary image* like the erecting ones of common spy-glasses.) I should, moreover, conceive that the same conditions would occur if achromatic glasses of similar force, and in equivalent states of correction, were substituted for the metals of the Gregorian telescope, and made to operate against each other on the same principle ; but this experiment I have never tried, either in telescopes or engyscopes.

I have in vain endeavoured to find an inverting eye-piece which would exert an influence over the state of spherical aberration of the object-glasses and metals of engyscopes. My way of judging was to form an image of an artificial star by their unassisted operation, as magnifiers in a solar opaque microscope, making the posterior conjugate focus to be of the same length as in an engyscope, that is, eight or nine inches, and having carefully examined the state of aberration in the picture, I then have attempted to modify it (when formed at the field bar of an engyscope) by various eye-glasses ; but so far as my eyes are capable of affording a criterion, I never succeeded in any degree. The image appeared as intractable as a picture formed by human hands, and remained inflexible both in its defects and excellencies, whatever they might be. The various eye-pieces I employed gave a better or worse field of view, were or were not achromatic, &c. *as regarded their own intrinsic operation* ; for example, they might give the oblique pencil in a better or worse state ; but if the said oblique pencil came in a confused or distorted state from the object-glass, I never could make them improve it. At the same time, I think it proper to observe, that I never tried thoroughly the effect of *positive achromatic eye-pieces in which there was a correcting concave lens of flint-glass*.

I shall now give the construction of an erecting eye-piece or engyscope, which is both achromatic and aplanatic to an aperture of about 27° , and having, moreover, a reasonable good correction of the oblique pencil. It is represented by Fig. 1. Plate I. *a* and *b* are two achromatic lenses; the focus of *b* is to that of *a* as two to one, and the interval between them is also equal to one; *c* is a stop placed immediately behind *b*, to regulate the aperture; *a* may be over-corrected for sphericity and dispersion; *b* under-corrected* for both, in such a manner that their aberrations shall neutralize each other, or each lens may be perfect in itself; *d e* is an Huygenian eye-piece. The interval *b d* must be regulated according to circumstances; but may be roughly stated as equal to that between the second and third glasses of a common erecting eye-piece of the same power.†

I have already stated that I consider the ordinary erecting eye-piece sufficiently perfect for practical purposes; but I think it would be a great improvement on spy-glasses if they could be shortened nearly one-half, so that they should be easily held and directed by the hand, when charged with a power of thirty or forty. I think an aperture of two inches and a half or three inches does as much upon terrestrial objects, at least in the day-time, as any other, the focal lengths of which are generally not less than thirty inches, which render the glass too long to be used without a stand. If the focal length was reduced to fifteen inches, the angular aperture would thereby be doubled, and with it the defining power also, (provided it could be perfectly executed.) In this case the erecting eye-piece I have given would be indispensable, for the colour of an ordinary one would become very sensible. Moreover, when we apply an erecting eye-piece to the object-glass of an engyscope, so as to obtain a low power, and for that end thrust it far in-

* Mr Lister has ascertained that an achromatic glass, having its inner surfaces in contact will vary in its spherical aberration according to circumstances. It matters not how or in what manner, or upon what principle, the aberrations of the two lenses are balanced.

† When this construction is used as an engyscope, there will be an abundant space left in front of the glass *a* for the illumination of opaque objects, provided its focus is not shorter than $\frac{4}{10}$ ths of an inch.

to the body, *so as to approach very near to the object-glass*, (in which case what I shall call its *posterior angle of aperture* becomes considerable,) a truly achromatic construction is also highly requisite.

Fig. 2 represents a positive inverting eye-piece for astronomical telescopes or for engyscopes used with micrometers; it is perfectly achromatic, and consists of two glasses, *a* and *b*, with a certain interval between them, for the purpose of improving the oblique pencil. They may be either achromatic and aplanatic in themselves or by mutual correction.* I am sorry to say that I have not been able to procure any combination which gives so flat a field of view as I could wish consistent with perfect distinctness. If it is thought worth while to obtain a large field at the expence of central perspicuity, we have only to reverse the position of the glasses, and that purpose will be accomplished.

I may observe, that if in these constructions the achromatic glasses have their inner surfaces in contact, and are cemented together, there will be no *sensible* loss of light beyond that occasioned by two common lenses.

I must now say somewhat about oblique pencils. They are very much mistaken who suppose that they have not received quite as much consideration as they deserve from those who have interested themselves of late in the improvement of the microscope. It is well known to every man practically acquainted with optics, and ought to be much better by those who know the science theoretically, that in all constructions whatsoever there is *a perpetual system of gaining advantages in one direction and losing them in another*, just as in mechanics,—what is gained in power is lost in time, &c. It is not permitted to man, assisted by the most powerful analysis, to combine together every advantage he may desire. He must content himself with what is attainable. According to the laws of the refraction and reflexion of light, I have ever found that all those constructions which give a very large angle of aperture combined with great distinctness, invariably produce what is called

* Their foci may be varied at pleasure, provided that the second or field-glass is not less than that of the first.

a bad field of view, that is to say, the vision is only perfect in the centre. In refracting instruments, a good deal may be done by combining several object-glasses together, more especially if they have intervals between them, (which, however, produce great inconvenience, by shortening the anterior conjugate focus, and thus precluding the use of high powers.) As the action of metals is very simple in comparison to that of lenses, I shall give an illustration of the mischief occasioned by what is called improving the oblique pencil in the Amician reflecting microscope. It must be evident, that, as the angular aperture is increased, the error of the oblique pencil must also be augmented; for it will come more and more oblique. Now it has actually been recommended to opticians to render the metals of the aforesaid instrument spherical, because in this case every pencil becomes equalized, and may be said to be equally bad or good, distinct or indistinct, throughout the whole field of view, though, so far as the objective part of the instrument is concerned, the whole instrument becomes utterly worthless if this figure is adopted. Those who work the metals of this instrument well know what infinite pains they have to get rid of this spherical figure and to attain the true one, * and that when they have done so, *the point is effected at the expence of a certain want of perspicuity about the edges of the field of view*, which is altogether *irremediable* with low powers, which take in a large portion of the marginal part of the image. If Gregorian and other telescopes, having a *large angular aperture*, are charged with a *low power*, the same defect is also very perceptible in them.

Of a piece with the performance of a spherical metal is that of a globular or bird's-eye object-glass, which is considered to produce an excellent correction of the oblique pencils, because they come *exactly like the centrical ones*, which are utterly uncorrected.

When the omnipotence of analysis shall point out some

* I have known Mr Cuthbert to have been employed a whole week in getting rid of nothing but the spherical error of one of his metals of $\frac{5}{10}$ ths of an inch focus and the same aperture, and not able to succeed at all in certain states of the weather, supposing always that a perfect figure was to be combined with a perfect polish.

truly achromatic and aplanatic construction which will give an oblique pencil as perfect as a central one, then will the labours of mathematicians have assumed a proper direction. At present I am afraid they have been hollowing before they are out of the wood. I trust, however, that the time will come when the subject will be taken up in the right point of view, or it will be likely to remain a standing *opprobrium mathematicorum*.

What could be a more beautiful and original subject for analysis than Mr Lister's discovery of the double aplanatic foci and variable aberration of achromatic object-glasses having their inner curves in contact? to say nothing of the admirable application of which it is capable. If I was a mathematician, I should feel my honour piqued that it had been made by experiment. It is quite plain that compound microscopes and erecting eye-pieces are only part and parcel of the subject of telescopes, and worthy of the same attention. Nothing which concerns the exact sciences can in my opinion be frivolous or unimportant,—a mere subtlety or conundrum having the charm of exactitude about it must always be respectable, though it may be of no positive utility.

A great deal has been said about the *insignificance* of microscopes, and that too much has already been written about them. Such opinions proceed naturally enough from the persons who are interested in opposing the new improvements, and are certainly highly worthy of a country where the exact sciences seem to be getting out of fashion, and where the experimental ones take precedence of them.

LAMBETH, April 1, 1831.

ART. VII.—*On the Specific Heats of some of the Metals, with an exposition of some erroneous Determinations by MM. Dulong and Petit; and an examination, as to the Metals, of the important law, that the ultimate particles of simple bodies have all the same capacity for Caloric.* By R. POTTER, Esq. Junior. Communicated by the Author.

IN the course of my optical inquiries, it became necessary for me to learn the capacities of steel and speculum metal for calo-

ric. The results of my experiments gave me numbers so much smaller than I had expected from a review of Mr Dalton's determinations for iron, copper, and tin, that I resolved to seize the opportunity of my first leisure time to enter into further investigations of the specific heats of the metals generally.

Mr Dalton has given a very extensive table of the specific heats of different substances, which in the metals are little different from the older determinations of Irvine, Wilcke, and Crawford; but these numbers are, I think, frequently too great by $\frac{1}{6}$ to $\frac{1}{10}$ part, for which allowance should be made when these tables are used.

MM. Dulong and Petit, who have so ably examined the phenomena of cooling bodies, presented also to the Academy of Sciences in 1819* an essay, of which the principal purport is the specific heats of certain of the metals determined by them, coupled with important theoretical considerations. In the course of their essay they take the opportunity to stigmatize as exceedingly erroneous, all the results of former chemists except those of their own countrymen. But before passing so sweeping a censure, it would have been as well if they had been sufficiently circumspect in their own experiments, to prevent a recoil of it with double force on their own labours. The method they made use of in this instance to find the capacities of the metals for caloric is the following:—The metal to be examined was reduced to a very fine powder, and pressed closely into a small and very thin cylinder of silver, having a thermometer placed in its axis. When the whole was adapted to a suitable apparatus, the capacity was deduced from the rate of cooling indicated by the thermometer; the quantity of even the heaviest metals required for the experiment being something less than an ounce English. It must be clear to every one, that, when we resort to intricate or *elegant* methods of experimenting, our vigilance and suspicion ought to be awakened, particularly when our results differ suddenly from the determinations of those who have used simpler apparatus; and it appears unaccountable, that, when MM. Dulong and Petit found their results for most of the metals of less capacity than silver differ so greatly from the determinations of Mr Dalton, of Wilcke, and of Crawford, they did not seek to prove them

* See *Annales de Chimie et de Physique* for 1819.

by the other mode, and also that they did not immediately consider the possible effect of their cylinder of silver, and of the bulb of the thermometer necessary to be used. As the matter stands, their table will be found to contain as great a proportion of error as any published on the subject.

In the method of determining the specific heats of solids not acted upon chemically by water, by immersing them in a known weight of that liquid at another temperature, and then noting the resulting temperature, it is clear that we have two ways of performing the experiment,—the one where the solid is hotter than the fluid, and the other the reverse; and the attendant errors, when we compare the results, are, as might be expected, generally in opposite directions, and may thus be rendered serviceable in pointing out the precautions necessary to be used, and the allowance to be made in deducing the correct determinations.

The great source of error in all experiments of this description is the alteration in temperature of the materials during the process of the experiment; for it is necessary that either the fluid or the solid body be at a temperature much above that of the air and surrounding objects. From the capacity of water for equal weights being so much greater than that of the metals, the influence of the alteration in temperature in producing error is much greater when the cold metal is immersed in tepid water than in the other way; and greater dexterity and more precautions are required.

In some of my first trials I used a vessel of tin-plate to contain the water, and obtained the following results:—

Quantity of the metal used.	Specific heat deduced from immersing the metal at 212° in an equal weight of water at common temps.		Specific heat deduced from immersing the metal at 55° in an equal weight of water at about 103°	
	For equal weights.	For equal bulks.	For equal weights.	For equal bulks.
Hard steel, 1lb. 11½ oz	.1026	.800		
Speculum metal, 6¾ oz.	.070	.62		
Hard steel, 2014 grs	.113	.8814	.200	1.56
Speculum metal, 2099 —	.075	.6675	.223	1.98

The great discordance in the results given by the reverse ways convinced me that the tin-plate vessel was an insufficient apparatus; and also in making use of the second method, or, adding the cool metal to the heated water, that considerable care and dexterity would be necessary to gain correct results. In lieu of the tin-plate vessel, my later experiments have been made with a compound one, consisting of an inner vessel of thin varnished linen surrounded by a close and friezed woollen cloth, and both these fixed in an earthen-ware one, of a dimension just sufficient to admit them. This compound vessel was capable of holding about 3500 grains of water. In making use of what I shall call the first method, I found this vessel quite sufficient, without need of any other particular precaution except that of going as quickly through the experiment as possible; but, in attempting the second method, I found the results still very unsatisfactory, until I adopted the plan of surrounding the compound vessel with water of nearly the temperature of that about to be experimented with, and also of preventing the evaporation from the surface by using a cover of varnished lincloth with the same sort of woollen cloth before-mentioned attached to it. In this manner, and by taking care to raise the cover no more than necessary when the metal was introduced, I got results which accorded as near with those got on the other method as I had any reason to expect. We might anticipate some little difference in the results of the two methods from the alterations in capacity arising from temperature; but a limit of error must be allowed much greater than any effect arising from this cause, between 32° and 212° Fahrenheit.

In taking the specific heats on the first method, it is necessary, when great correctness is wanted, to have the solid in one piece only, and presenting as small a surface as possible, to prevent to the utmost the loss of heat whilst it is in the course of being transferred from the boiling to the cold water. In this respect, MM. Dulong and Petit committed an oversight in experiments published in a previous paper to the one just alluded to; and it is on this account that I consider the numbers found for gold, silver, and copper, in the table below, on the first method, as much too small; because for these metals

I used British coins, where it was needful that there should be many pieces to have the requisite weight. This, it will be readily seen, is rather an advantage than a disadvantage in using the second method. I have moreover found, that, with a vessel of the capacity of the above, it was necessary to operate with at least about 1200 grains of metal to perform a good experiment. The numbers in the table for lead were obtained by immersing 8000 grains of the metal in 2000 grains of water. As this metal appears to have the smallest capacity of any metal known, I was anxious to determine it with accuracy, and particularly from having previously made several trials which did not accord very well together. When it can be practised, I should recommend future experimenters to follow the same plan, and use a greater weight of the substance which has the least capacity, more especially when they wish to obtain good approximations. These experiments show that .032 is very near the correct number for lead at common temperatures, as Mr Dalton's later experiments indicate.—See the Appendix to the first part of the second volume of his chemical work.

The thermometer which I have used I proved by breaking the column of mercury in the tube, and measuring the number of degrees which the separated length subtended in different parts of the scale, and found it very nearly correct between 32° and 212°.

	Weight used.	Number of pieces.	Specific heats from the first method, or the metal at 212° immersed in water at about 50°.	Specific heats from second method, or the metal at about 45° immersed in water at about 110°.
			For equal weights.	For equal weights.
Steel, (hard,)	2014 grs.	8	.1024	.115
—			.1086	
— (soft,)			.1080	
Zinc,	1932	1	.0912	.1034
—			.0948	
Copper,	2047	11	.0868	.1014
Silver,	1717	11	.0601	.0669
Tin, -	2111	2	.0596	.0542
— -	2107	1	.0597	.0588

Gold,	1414	12	.0375	.0509
Bismuth,	1650	3	.0390	.0393
Lead,	8000	32		.0328
—				.0336
—		1	.0307	

I have made several trials with wrought iron, which would indicate that there is a slight difference between its capacity and that of steel; but I am not yet prepared to speak positively on the amount of difference.

Reviewing the above table, I think we may conclude that the capacities in the first column of the following one are very near the truth *for common temperatures*. In the other columns I have given Mr Dalton's numbers, and those of MM. Dulong and Petit for the same metals.

	Specific heats from the fore- going table.	Specific heats according to Mr Dalton.	Specific heats according to MM. Dulong and Petit.
Iron,	.110	.13	0.1100
Zinc,	.098	.10	0.0927
Copper,	.096	.11	0.0949
Silver,	.063	.08	0.0557
Tin,	.056 .054?	.07	0.0514
Gold,	.046	.05	0.0298
Bismuth,	.039	.04	0.0288
Lead,	.032	.04 .032	0.0293

Here we see the great incorrectness of MM. Dulong and Petit's numbers for gold and bismuth, and how little room they had for pronouncing so decidedly on the labours of others. And if the relative weights of the atoms of the metals had been at the time sufficiently nearly ascertained, we see that there is no proportion of error in Mr Dalton's numbers, as they assert, which would have prevented him recognizing in them the law respecting the capacity for heat of the ultimate particles of gaseous bodies, which he has advanced in his new system of chemical philosophy. This law, which he appears to have noticed in several of the gases, he unfortunately expected to hold without modification in all, whether simple or compound.

ed, and had anticipations that similar laws would be found for liquids and solids generally.

It has since been maintained, on the ground of experiment, by the above-named French philosophers, that the law is only applicable, in the first instance, to the atoms of simple bodies, and requiring some modifications for those of compound ones which they had not been able to ascertain. The nature of caloric and its connexion with ponderable matter are still the subjects of so much uncertainty and dispute, that much more information will be necessary before we can arrive at even a tolerable view of its general properties, and the laws which they obey. But that Mr Dalton's proposition has a basis in nature there can be no reasonable doubt, though, even with MM. Dulong and Petit's modification and limitation, there is still ground for suspecting that we are not yet within sight of the general law. And to this effect Mr Dalton has a remark in the appendix to the 1st part of the 2d volume of the new system of chemical philosophy.

However, the law as enunciated by the French philosophers, "that the atoms of all simple bodies have the same capacity for heat," when applied to the class of the metals, holds, I find, with singular fidelity in COMMON TEMPERATURES, excepting in one very remarkable case, namely, that of silver. Whilst the exception remains, the law cannot be said to be established, much less can MM. Dulong and Petit in the least be said to have proved their proposition, as they have taken for the atomic weights numbers completely arbitrary, without referring either to the opinions of chemists or the results of analyses. Whence they obtained their numbers I shall now show. They appear to have appropriated such portions of Berzelius's numbers as suited their experiments. Thus, for bismuth they have taken $\frac{2}{3}$ of his number, for lead $\frac{1}{2}$, for gold $\frac{1}{2}$, for tin $\frac{1}{2}$, for silver $\frac{1}{4}$, for zinc $\frac{1}{2}$, for tellurium $\frac{1}{2}$, for copper $\frac{1}{2}$, for nickel $\frac{1}{2}$, for iron $\frac{1}{2}$, and for cobalt $\frac{1}{5}$ of his number. Where a theory of definite proportions only is referred to, such a proceeding might be warranted, but not so, on any ground, when atoms are considered without direct reference to analyses; and not having done this, they can only be said

to have shown the probability that such a law did exist in nature.

The proposition may be placed in view under the algebraic formula $\frac{c d}{m} = 1$, where c denotes the capacity for heat, taken with respect to some received standard, d is the relative atomic weight similarly considered, and m is a constant, whose value depends on the scales we adopt for the atomic weights and the capacities.

Taking, in the scale of capacities, that of water, for unity, and in that of the atoms the weights of hydrogen as unity, and oxygen 7, I find the value of m to be very nearly 2.75, and on these data I have calculated the weights of the atoms of the metals in the table below, where will be seen, in the respective columns, the numbers deduced from chemical analyses by Mr Dalton, Berzelius, and Dr Ure, and also those used by MM. Dulong and Petit, but the last, as well as those of Berzelius, I have reduced to the hydrogen scale, taking oxygen at 7. In Dr Ure's, 1-15th part must be allowed, as he adopts 7.5 as the weight of the atom of oxygen.

	Spec. heats for equal weight.	Weights of atoms for the formula $d = \frac{m}{c}$	Weights of the atoms according to	Weights used by Dulong and Petit.	
			Dalton.	Berzelius.	Ure.
Iron,	.110	25.	25.	47.49	28.
Zinc,	.098	28.	29.	56.45	34.
Copper,	.096	28.	56 or 28 ?	55.39	64.
Silver,	.063	44.	90.	189.22	110.
Tin,	.056	49.	52.	102.94	58.
Gold,	.046	60.	60. $\pm$	174.02	200.
Bismuth,	.039	70.	62.	124.16	72.
Lead,	.032	86.	90.	181.23	104.
					90.65

There is not here any more difference between the weights of the atoms deduced from the specific heats, and those deduced from analyses according to Mr Dalton, than must be allowed for the limit of error in either way, excepting in those for copper and silver, where the number from analyses is double the other. But in the analysis of the oxides and salts of the former metal, I see nothing which militates against the

supposition that the smaller number is the correct one, though Mr Dalton appears still to consider it doubtful. Dr Wollaston's equivalent for copper was exactly four times that of oxygen. In silver we are not entitled to draw a similar conclusion, and there is no analysis of an oxide or salt of silver which would entitle us to consider its atom half the weight Mr Dalton has it. Hence we are reduced to the alternative, that there is either another oxide of silver containing a larger proportion of oxygen than the one commonly known, and which has never yet been detected by chemists, or this metal must stand an exception to a law which holds so remarkably with the others. If, as I expect, the common oxide of silver will prove to be a sub-oxide, we shall here have an exemplification of the unexpected assistance which abstruse inquiries often lend to those branches of science where it was least anticipated.

11th May 1831.

ART. VIII.—*Observations on a new mode of constructing Harbours.* By WILLIAM MATHESON, Civil-Engineer. Communicated by the Author.*

THE importance of good harbours to a mercantile country will be universally admitted, and the commercial eminence great Britain has attained, has naturally directed the attention of persons interested, in the extension of her trade, to the improvement of the surrounding harbours; and perhaps in no way has the skill and ingenuity of engineers been more extensively employed, or more usefully directed. Many great works have been executed at an enormous expence, and some of them have succeeded more or less, to the expectation of the parties.

The erection of works within tide mark must always be attended with trouble and additional expence, and it is the more to be lamented after such expensive operations have been carried into effect, that the whole benefit of them should be frustrated by some unforeseen circumstance, which has too frequently happened in such operations.

Of these latent difficulties, none has had a more powerful influence than the tendency of the tide, or the motion of the

* Read before the Royal Society of Edin. 1831.

waters in particular situations, to silt or sand up spaces which have been enclosed, and upon which, by means of such inclosures, a change has been made upon the currents.

This tendency to sand up is peculiar to every situation, where quantities of sand are under the influence of the tide or the motion of the waters, and liable to be carried from place to place in mechanical combination with the water, either by means of the ebbing and flowing of the tide, or of the motion given to it by particular currents; or, by what is the most powerful of all, the motion given to it by the violence of the winds.

This motion of sand is generally in a direction opposite to that in which the wind is blowing, as may be seen in standing pools or lakes, where the sand, when the water is agitated by the wind, takes shelter under the banks of the weather side; but this will be more particularly explained afterwards.

In consequence of this tendency, the mouths of all rivers, and flat shores, and all bays and estuaries into which the tide rushes with violence, are more or less subject to the accumulation and shifting of sand banks, if sand is to be found in such situations. Examples of this may be seen by examining the harbours of Aberdeen, Dundee, Dublin, and a variety of others; and it may be generally asserted, that no harbour was ever erected upon a river that was not more or less subject to the inconvenience of shifting sand banks at the mouth of the river, or of being itself sanded up.

The writer of these observations having been employed at an early period of life in works connected with harbours, his attention was particularly directed more than thirty years ago, to the annoyance met with by the sanding up of harbours; and he saw the object frustrated, after the most expensive works had been erected, in the hope that they would answer every purpose expected from them.

Never losing sight of the evil which he had observed, nor ever ceasing to feel a desire to have it removed, he at last by mere accident, discovered in the harbour of Pulteney Town, a circumstance which led him to a corrective theory upon the subject.

An opening had been left in the inner pier for upwards of a year, while the other operations of building were going on, and

through which opening the receding tide, assisted by the land stream, passed into the harbour, and swept round and again passed out at its mouth. By this action of the tide, the harbour was kept perfectly free from any tendency to sand up; but in the course of a very short period, after the works were finished, and this opening closed up, in completing the harbour, the sand accumulated in it to such a degree as to make it inaccessible to vessels of even very small tonnage, and to cause the outlay of a large sum of money in the erection of an outer harbour which it is probable will be visited with the like obstructions, if they have not already taken place.

The practical hint thus afforded induced the writer to form the idea of erecting the necessary works for sea harbours, with artificial openings, so constructed as to enable the waters still to retain their natural motion, and by that means to avoid eddies and stagnation, by the latter of which the sand and alluvion are deposited, and by the former of which the deposition is shifted and whirled from place to place.

His plan, therefore, may be very shortly described. Wherever it may be necessary to erect a harbour, or to extend the works of one in a situation where sand or alluvion may be likely to accumulate, he proposes, after laying a solid foundation of stone, to make the next range of building consist of a succession of arches of such a height of opening as not to disturb any class of vessels that may frequent the harbour, while they are protected and sheltered by the solid building erected over these arches, and at the same time the openings shall be so proportioned to the depth of water, at the different points of the building, as to allow the tide to pass freely through.

To enter into any detail as to the particular mode of constructing the works or of forming the arches, &c. is quite unnecessary here, as these must vary according to circumstances. It is enough, if it is admitted that walls with arches may be constructed under water, as well as solid walls; because so much being granted, it will appear sufficiently obvious that the motion of the tide, either in approaching or receding, will naturally pass through these arches, and carry with it both in advancing and retiring, whatever is mechanically united with it, and the agitation

kept up upon the bottom of the harbour will prevent the sand being deposited.

There is another mode already hinted at of sand being collected, which the peculiar construction of harbours here described is equally well calculated to counteract. It is a fact well known to some of those who attentively observe the operations of nature, that all floating substances, especially such as do not rise above the surface of the water, approach the shore with a land wind, and recede from it by a contrary wind. This seems to arise from the violence of the wind upon the water causing the surface water to pass from the shore, and the under water to approach it, to supply its place by means of an opposite current. This under current is the more conspicuous in its operation, when the tide flows or ebbs in a contrary direction to that of the wind, and the floating substances carried along are accordingly deposited.

The consequence is, that, wherever large solid buildings are erected in a current of water, which has a tendency to sand up, and where these buildings pass along the shore, the wind from the shore that passes over them causes a sand bank to be formed seaward of the building, and to bring it directly into the mouth of the harbour, if it happens to be in the line of that building. This fact is strongly exemplified by the sand already accumulated at the back of the wet docks at Leith, or the stone pier at Newhaven, and in many other situations.

9, DEAN STREET, EDIN.

30th Nov. 1830.

ART. IX.—*On the Intensity of Light when the vibrations are elliptical.* By JAMES MACCULLAGH, Esq. Communicated by the Author.

ACCORDING to the opinions commonly received, the intensity of light, in the undulatory hypothesis, is proportional to the *vis viva*, which again is proportional to the square of the greatest velocity. Now the greatest velocities will be the same in an ellipse and a right line which have the same period, if the greater axis of the former be equal to the whole extent of the

latter ; so that in elliptic vibrations the intensity would be independent of the minor axis, which is far from being true. I would propose the integral $\int v^2 dt$ —so remarkable for its mechanical properties—as the measure of the intensity, the integral being extended to the whole time of a vibration. This gives precision to the notion of *vis viva*, and leads moreover to an elegant result ; for if a and b denote the semiaxes of the ellipse, and T the time of vibration, the integral, by an easy calculation, will be found equal to $\frac{2\pi^2}{T} (a^2 + b^2)$, showing that for the same colour the intensity is proportional to the sum of the squares of the semiaxes, and that for different colours it increases with the rapidity of the vibrations, as it would be natural to suppose *a priori*.

This theorem assigns very simply the reason why two portions of light polarized at right angles do not interfere ; but to show this, it will be necessary to lay down the following general rule for compounding rectilinear vibrations having the same period, whatever be the difference of their origins and directions.

Let AA' and BB' , Plate I. Fig 3, bisecting each other at O , represent in extent and direction the vibrations to be compounded, and suppose C and D to be two simultaneous positions of the moving molecule, which it would have in virtue of each vibration singly. Complete the parallelograms $O E$ and $O P$, and through P describe an ellipse having O for its centre, and touching the sides of the parallelogram $O E$; this ellipse will represent the resulting vibration ; it will have the same period as the component ones, and equal areas will be described in equal times about its centre.

To apply this construction to the case proposed, it is necessary to show that when OA and OB are constant and at right angles to each other, the intensity of the elliptic vibration, or the sum of the squares of the semiaxes, is independent of the difference of origin, or of the position of the points C and D .

Now in an ellipse when a perpendicular from the centre on a tangent makes an angle φ with the major axis, the square of its length is equal to $a^2 \cos^2 \varphi + b^2 \sin^2 \varphi$. If φ be the angle which

the major axis makes with O A, it will make its complement with O B, and we shall have

$$O A^2 = a^2 \cos^2 \varphi + b^2 \sin^2 \varphi,$$

$$O B^2 = a^2 \sin^2 \varphi + b^2 \cos^2 \varphi,$$

and therefore $O A^2 + O B^2 = a^2 + b^2$. Hence the intensity is independent of the difference of origin, and therefore the rays do not interfere.

This remarkable circumstance is commonly accounted for by observing, that since the component velocities are at right angles to each other, the square of the actual velocity must be the sum of their squares; but this proves the proposition only when the greatest velocities are simultaneous, which happens only in the cases of a complete accordance or of a difference of a semi-undulation in the interfering portions.

It may be observed also, that the method usually given for finding the intensity of the vibration resulting from two or more rectilinear vibrations proceeds upon no certain grounds. All the vibrations may be reduced to two in rectangular directions, the expression for the square of the velocity in each of those directions consisting of two parts, one of which is constant, and the other depends on the cosine of an arc increasing proportionally to the time; the square of the resultant velocity, therefore, consists also of a constant part, and a part depending similarly on the time, and it is assumed that the intensity of the resulting vibration is proportional to the former, which is true only in the very particular cases just mentioned, if the intensity be measured by the square of the greatest velocity.

This assumption, however, gives a correct result; and the reason that it does so is obvious from the principle laid down in the commencement: for if the expression for the square of the velocity be multiplied by the differential of the time and integrated, the variable part will vanish when the integral is extended to the whole time of a vibration.

ART. X.—*Extraordinary facts relating to the Vision of Colours: with Observations.* By Mr JOHN DALTON.*

It has been observed, that our ideas of colours, sounds, tastes,

* From *Memoirs of Lit. and Phil. Soc. of Manchester*, vol v. part i.

&c. excited by the same object may be very different in themselves, without our being aware of it ; and that we may nevertheless converse intelligibly concerning such objects, as if we were certain the impressions made by them on our minds were exactly similar. All, indeed, that is required for this purpose, is, that the same object should uniformly make the same impression on each mind ; and that objects which appear different to one should be equally so to others. It will, however, scarcely be supposed, that any two objects, which are every day before us, should appear hardly distinguishable to one person, and very different to another, without the circumstance immediately suggesting a difference in their faculties of vision ; yet such is the fact, not only with regard to myself, but to many others also, as will appear in the following account.

I was always of opinion, though I might not often mention it, that several colours were injudiciously named. The term *pink*, in reference to the flower of that name, seemed proper enough ; but when the term *red* was substituted for pink, I thought it highly improper ; it should have been *blue*, in my apprehension, as pink and blue appear to me very nearly allied ; whilst pink and red have scarcely any relation.

In the course of my application to the sciences, that of optics necessarily claimed attention ; and I became pretty well acquainted with the theory of light and colours before I was apprized of any peculiarity in my vision. I had not, however, attended much to the practical discrimination of colours, owing, in some degree, to what I conceived to be a perplexity in their nomenclature. Since the year 1790, the occasional study of botany obliged me to attend more to colours than before. With respect to colours that were *white*, *yellow*, or *green*, I readily assented to the appropriate term. *Blue*, *purple*, *pink*, and *crimson* appeared rather less distinguishable ; being according to my idea, all referable to *blue*. I have often seriously asked a person whether a flower was blue or pink, but was generally considered to be in jest. Notwithstanding this, I was never convinced of a peculiarity in my vision, till I accidentally observed the colour of the flower of the *Geranium zonale* by candle-light, in the autumn of 1792. The flower was pink, but it appeared to me almost an exact sky-blue by day ; in

candle-light, however, it was astonishingly changed, not having then any blue in it, but being what I called red, a colour which forms a striking contrast to blue. Not then doubting but that the change of colour would be equal to all, I requested some of my friends to observe the phenomenon ; when I was surprised to find they all agreed, that the colour was not materially different from what it was by day-light, except my brother, who saw it in the same light as myself. This observation clearly proved, that my vision was not like that of other persons ; and, at the same time, that the difference between day-light and candle-light, on some colours, was indefinitely more perceptible to me than to others. It was nearly two years after that time, when I entered upon an investigation of the subject, having procured the assistance of a friend, who, to his acquaintance with the theory of colours, joins a practical knowledge of their names and constitutions. I shall now proceed to state the facts ascertained under the three following heads :

I. An account of my own vision.

II. An account of others whose vision has been found similar to mine.

III. Observations on the probable cause of our anomalous vision.

I. *Of my own vision.*—It may be proper to observe, that I am short-sighted. Concave glasses of about five inches focus suit me best. I can see distinctly at a proper distance ; and am seldom hurt by too much or too little light ; nor yet with long application.

My observations began with the solar *spectrum*, or coloured image of the sun, exhibited in a dark room by means of a glass prism. I found that persons in general distinguish six kinds of colour in the solar image ; namely, *red, orange, yellow, green, blue, and purple*. Newton, indeed, divides the purple into *indigo* and *violet* ; but the difference between him and others is merely nominal. To me it is quite otherwise :—I see only *two* or at most *three* distinctions. These I should call *yellow* and *blue* ; or *yellow, blue, and purple*. My yellow comprehends the *red, orange, yellow, and green* of others ; and my *blue* and *purple* coincide with theirs. That part of the image which others call red, appears to me little more than a

shade, or defect of light; after that the orange, yellow, and green seem *one* colour, which descends pretty uniformly from an intense to a rare yellow, making what I should call different shades of yellow. The difference between the green part and the blue part is very striking to my eye: they seem to be strongly contrasted. That between the blue and purple is much less so. The purple appears to be blue much darkened and condensed. In viewing the flame of a candle by night through the prism, the appearances are pretty much the same, except that the red extremity of the image appears more vivid than that of the solar image.

I now proceed to state the results of my observations on the colours of bodies in general, whether natural or artificial, both by day-light and candle-light. I mostly used ribbands for the artificial colours.

Red by day-light.—Under this head I include *crimson*, *scarlet*, *red*, and *pink*. All crimsons appear to me to consist chiefly of dark blue; but many of them seem to have a strong tinge of dark brown. I have seen specimens of *crimson*, *claret*, and *mud*, which were very nearly alike. Crimson has a *grave* appearance, being the reverse of every shewy and splendid colour. Woollen yarn dyed crimson or dark blue is the same to me. *Pink* seems to be composed of nine parts of light blue, and one of red, or some colour which has no other effect than to make the light blue appear dull and faded a little. Pink and light blue, therefore, compared together, are to be distinguished no otherwise than as a splendid colour from one that has lost a little of its splendour. Besides the pinks, roses, &c. of the gardens, the following British *flora* appear to me blue; namely, *Statice Armeria*, *Trifolium pratense*, *Lychnis Flos-cuculi*, *Lychnis dioica*, and many of the *Gerania*. The colour of a florid complexion appears to me that of a dull, opaque, blackish blue, upon a white ground. A solution of sulphate of iron in the tincture of galls (that is, dilute black ink) upon white paper, gives a colour much resembling that of a florid complexion. It has no resemblance of the colour of blood. *Red* and *scarlet* form a genus with me totally different from pink. My idea of red I obtain from *vermilion*, *minium*, *scaling-wax*, *wafers*, *a soldier's uniform*, &c. These

seem to have no blue whatever in them. Scarlet has a more splendid appearance than red. Blood appears to me red ; but it differs much from the articles mentioned above. It is much more dull, and to me is not unlike that colour called *bottle-green*. Stockings spotted with blood or with dirt would scarcely be distinguishable.

Red by candle-light.—Red and scarlet appear much more vivid than by day. Crimson loses its blue and becomes yellowish red. Pink is by far the most changed ; indeed it forms an excellent contrast to what it is by day. No blue now appears ; yellow has taken its place. Pink by candle-light seems to be three parts yellow and one red, or a reddish yellow. The blue, however, is less mixed by day than the yellow by night. Red, and particularly scarlet, is a superb colour by candle-light ; but by day some reds are the least shewy imaginable : I should call them dark drabs.

Orange and yellow by day-light and candle-light.—I do not find that I differ materially from other persons in regard to these colours. I have sometimes seen persons hesitate whether a thing was white or yellow by candle-light, when to me there was no doubt at all.

Green by day-light.—I take my standard idea from grass. This appears to me very little different from red. The face of a laurel-leaf (*Prunus Lauro-cerasus*) is a good match to a stick of red sealing-wax ; and the back of the leaf answers to the lighter red of wafers. Hence it will be immediately concluded, that I see either red or green, or both, different from other people. The fact is, I believe that they both appear different to me from what they do to others. Green and orange have much affinity also. Apple green is the most pleasing kind to me ; and any other that has a tinge of yellow appears to advantage. I can distinguish the different vegetable greens one from another as well as most people ; and those which are nearly alike or very unlike to others are so to me. A decoction of Bohea tea, a solution of liver of sulphur, ale, &c. &c. which others call brown, appear to me green. Green woollen cloth, such as is used to cover tables, appears to me a dull, dark, brownish red colour. A mixture of two parts mud and one red would come near it. It resembles a red soil just turn-

ed up by the plough. When this kind of cloth loses its colour, as other people say, and turns yellow, then it appears to me a pleasant green. Very light green paper, silk, &c. is white to me.

Green by candle-light.—I agree with others, that it is difficult to distinguish greens from blues by candle-light; but, with me, the greens only are altered and made to approach the blues. It is the real greens only that are altered in my eye; and not such as I confound with them by day-light, as the brown liquids above-mentioned, which are not at all tinged with blue by candle-light, but are the same as by day, except that they are paler.

Blue by day-light and candle-light.—I apprehend this colour appears very nearly the same to me as to other people, both by day-light and candle-light.

Purple by daylight and candle-light.—This seems to me a slight modification of blue. I seldom fail to distinguish purple from blue; but should hardly suspect purple to be a compound of blue and red. The difference between day-light and candle-light is not material.

Miscellaneous observations.—Colours appear to me much the same by moon-light as they do by candle-light.

Colours viewed by lightning appear the same as by day-light; but whether exactly so, I have not ascertained.

Colours seen by electric light appear to me the same as by day-light. That is, pink appears blue, &c.

Colours viewed through a transparent sky-blue liquid, by candle-light, appear to me as well as to others the same as by day-light.

Most of the colours called drabs appear to me the same by day-light and candle-light.

A light drab woollen cloth seems to me to resemble a light green by day. These colours are, however, easily distinguished by candle-light, as the latter becomes tinged with blue, which the former does not. I have frequently seen colours of the drab kind, said to be nearly alike, which appeared to me very different.

My idea of *brown* I obtain from a piece of white paper heated almost to ignition. This colour by day-light seems to

have a great affinity to green, as may be imagined from what I have said of greens. Browns seem to me to be very diversified; some I should call red:—dark brown woollen cloth I should call black.

The light of the rising or setting sun has no particular effect; neither has a strong or weak light. Pink appears rather duller, all other circumstances alike, in a cloudy day.

All common combustible substances exhibit colours to me in the same light; namely, *tallow, oil, wax, pit-coal.*

My vision has always been as it is now:

II. *An account of others whose vision has been found similar to mine.*—It has been already observed that my brother perceived the change in the colour of the geranium such as myself. Since that time having made a great number of observations on colours, by comparing their similarities, &c. by day-light and candle-light, in conjunction with him, I find that we see as nearly alike as any other persons do. He is shorter sighted than myself.

As soon as these facts were ascertained, I conceived the design of laying our case of vision before the public, apprehending it to be a singular one. I remembered, indeed, to have read in the *Philosophical Transactions* for 1777, an account of Mr Harris of Maryport in Cumberland, who, it was said, “could not distinguish colours;” but his case appeared to be different from ours. Considering, however, that one anomaly in vision may tend to illustrate another, I reperused the account; when it appeared extremely probable that, if his vision had been fully investigated, and a relation of it given in the first person, he would have agreed with me. There were four brothers in the same predicament, one of whom is now living. Having an acquaintance in Maryport, I solicited him to propose a few queries to the survivor, which he readily did, (in conjunction with another brother, whose vision has nothing peculiar,) and from the answers transmitted to me, I could no longer doubt of the similarity of our cases. To render it still more circumstantial, I sent about twenty specimens of different coloured ribbands, with directions to make obser-

vations upon them by day-light and candle-light: the result was exactly conformable to my expectation.

It then appeared to me probable, that a considerable number of individuals might be found whose vision differed from that of the generality, but at the same time agreed with my own. Accordingly, I have since taken every opportunity to explain the circumstances amongst my acquaintance, and have found several in the same predicament. Only one or two I have heard of who differ from the generality and from us also. It is remarkable that, out of twenty-five pupils I once had, to whom I explained this subject, two were found to agree with me; and, on another similar occasion, one. Like myself, they could see no material difference betwixt pink and light blue by day, but a striking contrast by candle-light. And, on a fuller investigation, I could not perceive they differed from me materially in other colours. They, like all the rest of us, were not aware of their actually seeing colours different from other people; but imagined there was great perplexity in the *names* ascribed to particular colours. I think I have been informed already of nearly twenty persons whose vision is like mine. The family at Maryport consisted of six sons and one daughter; four of the sons were in the predicament in question. Our family consisted of three sons and one daughter who arrived at maturity; of whom two sons are circumstanced as I have described. The others are mostly individuals in families, some of which are numerous. I do not find that the parents or children in any of the instances have been so, unless in one case. Nor have I been able to discover any physical cause whatever for it. Our vision, except as to colours, is as clear and distinct as that of other persons. Only two or three are short-sighted. It is remarkable that I have not heard of one female subject to this peculiarity.

From a great variety of observations made with many of the above-mentioned persons, it does not appear to me that we differ more from one another than persons in general do. We certainly agree in the principal facts which characterize our vision, and which I have attempted to point out below. It is but justice to observe here, that several of the resemblances and comparisons mentioned in the preceding part of this

paper were first suggested to me by one or other of the parties, and found to accord with my own ideas.

Characteristic facts of our vision.—1. In the solar spectrum three colours appear, yellow, blue, and purple. The two former make a contrast; the two latter seem to differ more in degree than in kind.

2. *Pink* appears, by day-light, to be sky-blue a little faded; by candle-light it assumes an orange or yellowish appearance, which forms a strong contrast to blue.

3. *Crimson* appears a muddy blue by day; and crimson woollen yarn is much the same as dark blue.

4. *Red* and *scarlet* have a more vivid and flaming appearance by candle-light than by day-light.

5. There is not much difference in colour between a stick of red sealing-wax and grass, by day.

6. Dark green woollen cloth seems a muddy red, much darker than grass, and of a very different colour.

7. The colour of a florid complexion is dusky blue.

8. Coats, gowns, &c. appear to us frequently to be badly matched with linings, when others say they are not. On the other hand, we should match crimsons with claret or mud; pinks with light blues; browns with reds; and drabs with greens.

9. In all points where we differ from other persons, the difference is much less by candle-light than by day-light.

III. *Observations tending to point out the cause of our anomalous vision.*—The first time I was enabled to form a plausible idea of the cause of our vision, was after observing that a sky-blue transparent liquid modified the light of a candle so as to make it similar to day-light; and, of course, restored to pink its proper colour by day, namely, light blue. This was an important observation. At the same time that it exhibited the effect of a transparent coloured medium in the modification of colours, it seemed to indicate the analogy of solar light to that resulting from combustion; and that the former is modified by the transparent blue atmosphere, as the latter is by the transparent blue liquid. Now the effect of a transparent coloured

medium, as Mr Delaval has proved, is to transmit more, and consequently imbibe fewer of the rays of its own colour, than of those of other colours. Reflecting upon these facts, I was led to conjecture that one of the humours of my eye must be a transparent, but *coloured*, medium, so constituted as to absorb *red* and *green* rays principally, because I obtain no proper ideas of these in the solar spectrum; and to transmit blue and other colours more perfectly. What seemed to make against this opinion, however, was, that I thought red bodies, such as vermilion, should appear black to me, which was contrary to fact. How this difficulty was obviated will be understood from what follows.

Newton has sufficiently ascertained, that opaque bodies are of a particular colour from their reflecting the rays of light of that colour more copiously than those of the other colours; the unreflected rays being absorbed by the bodies. Adopting this fact, we are insensibly led to conclude, that the more rays of any one colour a body reflects, and the fewer of every other colour, the more perfect will be the colour. This conclusion, however, is certainly erroneous. Splendid coloured bodies reflect light of every colour copiously; but that of their own most so. Accordingly we find, that bodies of all colours, when placed in homogeneal light of any colour, appear of that particular colour. Hence a body that is red may appear of any other colour to an eye that does not transmit red, according as those other colours are more copiously reflected from the body, or transmitted through the humours of the eye.

It appears, therefore, almost beyond a doubt, that one of the humours of my eye, and of the eyes of my fellows, is a *coloured* medium, probably some modification of blue. I suppose it must be the vitreous humour; otherwise I apprehend it might be discovered by inspection, which has not been done. It is the province of physiologists to explain in what manner the humours of the eye may be coloured, and to them I shall leave it; and proceed to show that the hypothesis will explain the facts stated in the conclusion of the second part.

1. This needs no further illustration.

2. *Pink* is known to be a mixture of red and blue; that is, these two colours are reflected in excess. Our eyes only trans-

mit the blue excess, which causes it to appear blue; a few red rays pervading the eye may serve to give the colour that faded appearance. In candle-light, red and orange, or some other of the higher colours, are known to abound more proportionably than in day-light. The orange light reflected may therefore exceed the blue, and the compound colour consist of red and orange. Now, the red being most copiously reflected, the colour will be recognized by a common eye under this small modification; but the red not appearing to us, we see chiefly the orange excess; it is consequently to us not a modification but a new colour.

3. By a similar method of reasoning, *crimson*, being compounded of red and dark blue, must assume the appearances I have described.

4. Bodies that are red and scarlet probably reflect orange and yellow in greatest plenty, next after red. The orange and yellow, mixed with a few red rays, will give us our idea of red, which is heightened by candle-light, because the orange is then more abundant.

5. Grass-green is probably compounded of green, yellow, and orange, with more or less blue. Our idea of it will then be obtained principally from the yellow and orange mixed with a few green rays. It appears, therefore, that red and green to us will be nearly alike. I do not, however, understand, why the greens should assume a bluish appearance to us and to every body else, by candle-light, when it should seem that candle-light is deficient in blue.

6. The green rays not being perceived by us, the remaining rays may, for aught that is known, compound a muddy red.

7. The observations upon the phenomena of pink and crimson will explain this fact.

8. Suppose a body to reflect red rays as the number 8, orange rays as the number 6, and blue as 5; and another body red 8, orange 6, and blue 6: then it is evident that a common eye, attending principally to the red, would see little difference in those colours; but we, who form our ideas of the colours from the orange and blue, should perceive the latter to be bluer than the former.

9. From the whole of this paper it is evident, that our eyes

admit blue rays in greater proportion than those of other people; therefore, when any kind of light is less abundant in blue, as is the case with candle-light compared to day-light, our eyes serve in some degree to temper that light, so as to reduce it nearly to the common standard. This seems to be the reason why colours appear to us by candle-light, almost as they do to others by daylight.

I shall conclude this paper by observing, that it appears to me extremely probable, that the sun's light and candle-light, or that which we commonly obtain from combustion, are originally constituted alike; and that the earth's atmosphere is properly a *blue fluid*, and modifies the sun's light so as to occasion the commonly perceived difference.

ART. XI.—*On the relation between the Refractive Powers and the polarizing angles of singly refractive substances.* By Dr A. SEEBECK. Translated from Poggendorff's *Annalen*, 1830, No. 9, p. 27.

WHEN Malus discovered that a ray of light reflected from the surface of bodies not metallic, possessed the property of being *polarized* like one of the pencils formed by calcareous spar, he sought in vain to discover a relation between the angles at which this property was most completely produced and the other properties of the reflecting body. He observed that the angle of maximum polarization was in general greater with bodies of a great refraction, and that the pencil reflected from both surfaces was simultaneously polarized; but notwithstanding these observations, he expressly says, "I have determined with many substances the angle at which light is completely polarized, and I have found that this angle follows neither the order of the refractive powers nor that of the dispersive forces. It is a property of bodies independent of the other modes of action which they exercise upon light."

It must, therefore, have been a matter of great surprise when Dr Brewster announced in the *Philosophical Transactions* for 1815, as the results of his measurements, that the angle of maximum polarization is a function of the refractive power of the reflecting body. This law he expresses thus :

The index of refraction is the tangent of the angle of polarization ; or according to his other mode of expressing it. When the polarization is complete, the reflected ray forms a right angle with the reflected ray. Dr Brewster arranges his measurements in a table which contains the following eighteen substances, viz. air, water, fluor-spar, obsidian, bird-lime, sulphate of lime, rock crystal, opal coloured glass, topaz, mother-of-pearl, Iceland-spar, orange coloured glass, spinelle ruby, zircon, glass of antimony, sulphur, diamond, chromate of lead. By comparing the observed with the calculated angles of polarization, he found that the difference varied from $+25'$ to $-32'$, the sum of the eighteen positive differences being $85'$ and that of the negative ones $174'$, which does not seem inconsiderable when we consider that the observed property is the mean of a number of observations.

These differences are, nevertheless, not to be ascribed to accidental mistakes of observation, for the table contains six or seven doubly refracting bodies ; and Dr Brewster has himself shown in a later memoir in the *Philosophical Transactions* for 1819, that in this class of bodies the angle of polarization varies with the inclination of the reflecting surface to the axis of the crystal. These variations, indeed, are so considerable, that they explain the cause of many more glaring differences than those in the table referred to.

With respect to a particular class of singly refracting substances, viz. the glasses, Dr Brewster found very considerable deviations from the law of the tangents ; but he regards them as only apparent exceptions, as he found that they were owing to chemical alterations on the surface, which changed the refractive power of the surface without changing that of the remaining mass, so that the angle of polarization observed on the surface cannot correspond to the refractive power of the mass.

It is evident, therefore, that in such cases the angle of polarization should be measured on newly polished surfaces of glass ; but Dr Brewster does not say that he has made any experiments of this kind.

Under these circumstances, it did not seem to me superfluous to resume the inquiry, limiting it to singly refracting substances, and directing my attention particularly to the surfaces

of glass, with the view of reconciling Dr Brewster's law to this class of bodies, by accurate measurements of the refractive power and of the angle of polarization in the same pieces.

(Dr Seebeck then proceeds to describe the divided instrument by which he measured the angles of refraction, from which he deduced the following measures of the refractive power.)

Colourless fluor-spar,	-	1.4341	Index of refraction.
Greenish blue fluor-spar,		1.4343	
Common opal,	-	1.4516	
Glass A colourless Eng. plate glass,		1.5130	
— B colourless plate glass,		1.5266	
— C English crown glass,		1.5321	
— D do. do. -		1.5523	
— E English flint-glass,		1.5783	
— F do. do.		1.6206	
Pyrope,	-	1.8131	
Yellow blende,	-	2.3692	

After describing the instrument for measuring the angle of polarization upon these substances, which does not differ much from those previously used for the same purpose, Dr Seebeck gives the following table of his results. The surfaces were mostly polished about six and eight months before, and the reflecting surfaces on which the polarizing angle was measured were those by which the ray was refracted when the refractive power of the substance was determined. The differences between the observed and the calculated angles seldom amount to above 20', and in very few to above 30'. The calculated angle of polarization given in the table is that deduced from Dr Brewster's law.

Differences.

1. *Colourless fluor-spar.*

Calculated angle of polarization	-	55° 7'	
Observed do. on 1 surface	-	55 9	+0° 2
— on 2 do.	-	55 6	-0° 1

2. *Bluish fluor-spar.*

Calculated angle of polarization		55 7	
Observed do. 1 surface	-	55 14	+0 7

Observed angle, 2 surface	-	55 9	+0 2
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3. *Common opal.*

Calculated angle of polarization		55 26	
Observed do. on 1 surface	-	55 39	+0 13
----- 2 do.	-	55 41	+0 15

4. *Glass A.*

Calculated angle of polarization		56 32	
Observed do. 1 surface	-	56 32	0 0
----- 2 do.	-	56 46	+0 14

5. *Glass B.*

Calculated angle of polarization		56 96	
Observed do. 1 surface	-	56 37	-0 9
----- 2 do.	-	56 36	-0 10

6. *Glass C.*

Calculated angle of polarization,	-	56 52	
Observed do. on 1 surface,	-	57 6	+0 14
----- 2 do.	-	57 3	+0 11
----- 3 do.	-	57 14	+0 22

7. *Glass D.*

Calculated angle of polarization,	-	57 13	
Observed do. on 1 surface,	-	57 19	+0 6
----- 2 do.	-	57 20	+0 7

8. *Glass E.*

Calculated angle of polarization,	-	57 38	
Observed do. on 1 surface,	-	57 15	-0 23
----- 2 do.	-	57 0	-0 38

9. *Glass F.*

Calculated angle of polarization,	-	58 19	
Observed do. on 1 surface,	-	58 12	-0 7
----- 2 do.	-	58 17	0 2
----- 3 do.	-	58 8	-0 11

10. *Pyrope.*

Calculated angle of polarization,	-	61 8	
Observed do. on 1 surface,	-	60 28	-0 40
----- 2 do.	-	60 42	-0 26

11. *Yellow Blende.*

Calculated angle of polarization,	-	67 7	
Observed do. on natural surface,	-	66 46	-0 21
----- artificial do.	-	66 35	-0 32

In most of these cases, the differences are too great to be ascribed to accidental errors of observation, and it was not probable that they arose from Dr Brewster's law being inexact, or susceptible of modification, because the differences were in some cases greater in two surfaces of the same body than might be expected from accidental errors. I therefore determined to repeat the measurements on a newly ground and polished surface of the glass E, where the differences were greatest, viz. $23'$ and $38'$. The observed angle of polarization, by a mean of twenty observations, was now $57^{\circ} 40'.6$, differing only $-2'.1$ from $57^{\circ} 38'.5$, the calculated angle. I now ground and re-polished the surface of the same prism which had before given a difference of $-38'$, and found it to be $57^{\circ} 41'.4$, the difference being only $+0^{\circ} 2'.9$

As these small differences are clearly within the limits of accidental errors of observation, I re-ground and polished all the other substances in the preceding table, and having re-measured the angle of polarization, I found it to agree so perfectly with Dr Brewster's law, that the slight deviations from it could be ascribed only to errors of observation. The results of these measurements are given in the following table.

The angle of polarization for the glass E, is the mean of the two measurements already mentioned. The angle for pyrope is the mean of twenty measurements upon one newly ground face, which was $61^{\circ} 3'.3$, and of twenty measurements upon another newly ground face, which was $61^{\circ} 4'.8$. The angle for blende was observed in two natural cleavage planes; the mean of twenty observations in one being $67^{\circ} 8'.2$, and of twenty in the other, $67^{\circ} 8'.1$. The angle for the other substances was the mean of twenty observations upon surfaces newly ground and polished.

Colourless Fluor

spar,	55° 6'.7 Calc.	55° 6'.7 Obs.	0° 0'.0 Diff.
Bluish Fluor spar	55 7.0	55 3.8	— 0 3.2
Common opal,	55 26.3	55 29.3	+ 0 3.0
Glass A,	56 32.2	56 36.0	+ 0 3.8
— B,	56 46.4	56 45.5	— 0 0.9
— C,	56 52.0	56 50.2	— 0 1.8
— D,	57 12.6	57 12.6	0 0.0

Glass E,	57 38.5	57 41.0	+ 0 2.5
— F,	58 19.4	58 16.6	— 0 2.8
Pyrope,	61 7.7	61 4.0	— 0 3.7
Yellow blende,	67 7.0	67 8.2	+ 0 1.2

The great agreement between the calculated and observed results, as given in this table, confirms, in the case of singly refracting substances, the accuracy of Dr Brewster's law, that the tangent of the angle of polarization is the index of refraction,—a law with which observation will coincide only when the reflecting surface has the same property as the rest of the mass.

What the cause is which produces the change upon the outer surface of the glass has not been determined by my experiments. That there is a chemical alteration produced by the air only, particularly on the surfaces of glass, is highly probable, yet I do not maintain that this is the principal cause of the observed anomalies. Upon newly polished surfaces of the glass C, where the difference was at first $-0^{\circ} 1'.8$, I found the mean of twenty measurements of the polarizing angle to be after *six weeks* $56^{\circ} 54'.6$; after *fourteen weeks*, $56^{\circ} 51'.8$; after six months, $56^{\circ} 54'.0$, the differences $+ 0^{\circ} 2'.6$, $- 0^{\circ} 0'.2$ and $+ 0^{\circ} 2'.0$ being within the limits of errors of observation, so that the upper surface does not seem to have suffered any change in that period. When I had the surfaces of the glass E, which formerly gave angles which agreed with the calculation, again polished by an artist, they gave an angle of $58^{\circ} 7'.0$, which gave a difference of $+ 0^{\circ} 28'.5$, whereas the differences were originally $- 0^{\circ} 23'$, and $- 0^{\circ} 38'$. Hence it can scarcely be doubted, that this great variation arose from some mechanical cause, produced perhaps by the cleaning of the surface; and it is probable, that the same cause produced the other deviations from Dr Brewster's law.

OBSERVATIONS BY THE EDITOR.

The preceding observations of Dr Seebeck are undoubtedly the most accurate that have been made on the polarizing angles of particular surfaces, and are exceedingly valuable even as insulated facts, independent of their confirmation of the law of the tangents. This law, however, had been established by many observations of my own, subsequent to the publication

of my paper in 1815, and also by observations of MM. Arago and Biot ; and it has been adopted by Fresnel as a law rigorously physical, and enters into the beautiful formulæ with which he has enriched the science of optics. It derived also additional support from the numerous observations made by M. Arago and myself, on the change in the plane of polarization produced by reflexion.

The results of Dr Seebeck respecting the cause of the superficial changes in glass and other bodies, are, as he allows, not perfectly satisfactory. I had shown in my first paper, (*Phil. Trans.* 1815, p. 124, 125,) that the anomalies in glass arose from a chemical change in the surface, which was sometimes obvious even to the eye, and from oxidations produced by the action of the air, and by heat. In a subsequent paper on a different class of phenomena, (See *Phil. Trans.* 1829, and this *Journal*, No. ii. N. Series, p. 209, &c.) I have shown how to detect these oxidations when they are wholly invisible to the eye ; and I have proved that they take place on some kinds of glass, and not on others. It would be desirable, therefore, that Dr Seebeck should repeat his experiments with the glass E, and examine the state of its surface when it gives different angles of polarization, by examining the pencil reflected from these surfaces, when they are covered by an oil of the same refractive and dispersive power. A slight absorption of some of the minutely divided matter, used in grinding and polishing the surface, may account for the anomalies in question in those cases, if any really exist, where they are not produced by oxidation.

ART. XII.—*Abstract of Meteorological Observations made in the Isle of Man from 1822 to 1831.* By ROBERT STEWART, Esq. Receiver-General of the Isle of Man. Communicated by Dr HIBBERT.

IN Number X. of this *Journal*, p. 231, we have already given an abstract of Mr Stewart's valuable observations made from 1822 till 1826. The thermometer was situated in a northern exposure, and was observed at 9 o'clock A. M. and 11 o'clock P. M. The following are the observations made since 1825.

1826.

	Thermometer.		Wind. Number of days.			
	A. M.	P. M.	N.	S.	E.	W.
January,	39	35 $\frac{1}{2}$	7	11	8	5
February,	44 $\frac{1}{2}$	43	4	13	1	10
March,	43	44 $\frac{1}{2}$	6	5	14	6
April,	48	43	11	4	2	13
May,	53 $\frac{1}{2}$	50	6	2	19	4
June,	60	55	11	9	7	3
July,	66 $\frac{1}{2}$	63	6	11	4	10
August,	67 $\frac{1}{2}$	62 $\frac{1}{2}$	1	11	1	18
September,	58 $\frac{1}{2}$	53	4	8	12	6
October,	55 $\frac{1}{2}$	49 $\frac{1}{2}$	8	5	4	14
November,	43 $\frac{1}{2}$	42	10	0	11	9
December,	44 $\frac{1}{2}$	44	8	7	4	12
Gen. Medium,	53 $\frac{1}{4}$	49 $\frac{1}{4}$	82	86	87	110

	A. M.	P. M.
Highest state of thermometer,	75	70
Lowest,	30	24
Mean,	51 $\frac{1}{4}$ °	

Weather and Rain.				
Number of Days.				Inches of
	Rain.	Snow.	Fair.	rain.
January, -	7	1	23	0.79
February, -	17	2	9	4.75
March, -	6	3	22	1.79
April, -	10	1	19	2.59
May, -	3	0	28	0.52
June, -	3	0	27	0. 4
July, -	10	0	21	1.30
August, -	14	0	17	2.35
September,	11	0	19	2. 4
October, -	17	0	14	3.55
November, -	11	4	15	3.52
December, -	17	1	13	6.28
Gen. Medium, 126	12	227		29.52

1827.

	Thermometer.		Wind. Number of days.			
	A. M.	P. M.	N.	S.	E.	W.
January, -	39	38 $\frac{1}{2}$	12	4	7	8
February, -	39	37	9	4	14	1
March, -	42	41	9	2	2	18
April, -	47	45	5	5	13	7
May, -	52	49	4	17	7	3
June, -	57	53	8	12	6	4
July, -	60	55	11	8	8	4
August, -	58	55	11	6	12	2
September, -	59	55	5	12	10	3
October, -	54	52	8	11	12	0
November, -	48	48	16	6	5	3
December, -	46	45	7	6	6	12
Gen. Medium,	50	48	105	93	102	65

	A. M.	P. M.
Highest state of thermometer, -	70	64
Lowest, -	25	25
Mean, -	49°	

	Weather and Rain. Number of days.			Inches of rain.
	Rain.	Snow.	Fair.	
January, -	13	5	13	4.58
February, -	5	1	22	2.12
March, -	15	5	11	5.65
April, -	9	2	19	2.41
May, -	14	0	17	1.76
June, -	11	0	19	2.79
July, -	10	0	21	2.65
August, -	11	0	20	4.58
September, -	12	0	18	2.12
October, -	15	0	16	3.50
November, -	16	1	13	4.33
December, -	21	1	9	6.92
Gen. Medium,	152	15	198	43.41

1828.

	Thermometer.		Wind.			
	A. M.	P. M.	N.	Number of days.		
				S.	E.	W.
January, -	43	42	6	8	12	5
February, -	41	42	7	12	6	4
March, -	41	43	14	5	6	6
April, -	47	44	6	11	8	5
May, -	54	51	3	3	17	8
June, -	58	55	2	10	8	10
July, -	58	59	0	18	3	10
August, -	61	58	4	8	11	8
September, -	59	56	2	12	14	2
October, -	53	51	6	10	7	8
November, -	50	48	8	4	11	7
December, -	49	48	8	10	4	9
Gen. Medium, 51	49		64	113	107	82

	A. M.	P. M.
Highest state of thermometer, -	67	64
Lowest, - - -	33	32
Mean, - - -	50°	

Weather and Rain.				
Number of days.				
	Rain.	Snow.	Fair.	Inches of rain.
January, -	11	2	18	3.47
February, -	13	2	14	2.56
March, -	7	3	21	1.80
April, -	15	1	14	4.38
May, -	10	0	21	2 5
June, -	10	0	20	1 63
July, -	12	0	19	3.33
August, -	9	0	22	2. 9
September, -	10	0	20	2.50
October, -	14	0	17	2.31
November, -	17	1	12	4.72
December, -	19	0	12	5. 6
Gen. Medium, 147	9		210	35.90

1829.

Thermometer.

Wind.

Number of days.

	A. M.	P. M.	N.	S.	E.	W.
January, -	36	41	12	2	17	0
February, -	43	43	6	2	12	8
March, -	41	40	3	6	22	0
April, -	46	42	6	8	11	5
May, -	53	50	4	15	7	5
June, -	60	54	8	8	6	8
July, -	60	55	9	15	4	3
August, -	57	54	18	3	3	7
September, -	52	49	13	8	2	7
October, -	49	47	12	6	7	6
November, -	44	45	6	4	8	12
December, -	40	39	5	14	11	1
Gen. Medium, 48	46		102	91	110	62

	A. M.	P. M.
Highest state of thermometer, -	65	62
Lowest, - - -	28	27
Mean, - - -	47°	

Weather and Rain.				Inches of rain.
Number of days.				
	Rain.	Snow.	Fair.	
January, -	4	7	20	0.87
February, -	9	1	18	1.58
March, -	4	0	27	1. 0
April, -	15	2	13	3.10
May, -	5	0	26	1.53
June, -	11	0	19	1.77
July, -	18	0	13	2.86
August, -	17	0	14	5.80
September, -	17	0	13	3.65
October, -	15	0	16	6.10
November, -	14	1	15	3.81
December, -	6	2	23	1.82
Gen. Medium, 135	13	217		33.89

1830.

Thermometer.				Wind. Number of days.			
	A. M.	P. M.	N.	S.	E.	W.	
January, -	39 $\frac{1}{2}$	37	17	7	5	2	
February, -	38	37	5	11	5	7	
March, -	44 $\frac{1}{2}$	47	5	7	6	13	
April, -	47	45	4	8	7	11	
May, -	52	49	2	11	13	5	
June, -	55	53	7	10	3	10	
July, -	56	57	5	15	4	7	
August, -	58	52	6	7	8	10	
September, -	54	52	3	10	3	14	
October, -	52	49	8	4	12	7	
November, -	49	45	5	7	11	7	
December, -	39	38	16	7	7	1	
Gen. Medium, 48.8				83	104	84	94

	A. M.	P. M.
Highest state of thermometer, -	66	67
Lowest, - - - -	25	26
Mean, - - - -	47°.8	

Weather and Rain.				Inches of rain.	
Number of days.					
		Rain.	Snow.	Fair.	
January,	-	5	4	22	0.73
February,	-	8	7	13	2.40
March,	-	12	0	19	1.95
April,	-	14	2	14	3.72
May,	-	19	0	12	3.94
June,	-	22	0	8	2.67
July,	-	17	0	14	3.61
August,	-	12	0	19	2.67
September,	-	17	0	13	6.83
October,	-	15	0	16	3.42
November,	-	14	0	16	3.74
December,	-	12	2	17	2.87
Gen. Medium, 167		15	183		38.55

From these and the former observations of Mr Stewart, we obtain the following results:

1822,	-	-	-	51.5
1823,	-	-	-	46.1
1824,	-	-	-	49.8
1825,	-	-	-	50.7
1826,	-	-	-	51.3
1827,		-	-	49
1828,	-	-	-	50
1829,	-	-	-	47
1830,	-	-	-	47.8

Mean Temp. for 9 years,	-	-	-	49.24
Correction in consequence of 9 ^h , and 11 ^h , not being the				
hours of Mean Temperature,	-	-	-	-0.84

48.40

Mean Temperature according to Dr Brewster's for-				
mula,	-	-	-	-47.58

Difference between the formula and observation, + 0.82

It is curious to remark, that the mean temperature of the *four* years, 1822-1825, inclusive, is precisely the same as that of the *nine* years, 1822-1830, inclusive.

ART. XIII.—*Account of an Excursion from Lima to Pasco.*

In a Letter from ALEXANDER CRUCKSHANKS, Esq. to Dr HOOKER.

THIS interesting letter, of which we propose to present an abridgement to our readers, is published in Dr Hooker's *Botanical Miscellany*, (Parts v. vi. p. 177-198, for March 1831,) a valuable and instructive work, the botanical details of which are relieved and enlivened by much amusing personal narrative. We must refer the botanical reader to the original letter for the details, in which he will take the greatest interest.

"Having determined to accompany my friend Mr M——, an English merchant of Lima, who had been appointed Director of a Company for draining the mines, we left the capital at noon on the 21st of June. Our party consisted of six individuals, and

we had with us three English workmen belonging to the Company, a servant, and three muleteers : eight laden mules completed the cavalcade.

“ On leaving Lima, the road proceeds for about a league towards the coast, to a place called Arnipukuio, and then, rounding the hills that separate the two vallies, enters that of Chillon and turns inland. The *Scotch-misty* weather, the winter of the coast, had lately set in, and the hills were beginning to show the effects of the moisture, being thinly clad with verdure wherever a covering of soil had accumulated on the rock. About half a league out of Lima in this direction, there is a little valley bounded by very high hills, called *Los Amancaes*, from the vast number of these flowers that appear there at the beginning of winter. The great height of the hills encourages a plentiful deposition of moisture, which produces a more abundant vegetation than is usually seen on the coast. After the rains have ceased in the interior, the Indians who rear cattle there are accustomed to drive them down to different parts of the hills and vallies in the low country, till they reach the coast ; and at this season, a considerable number of small flocks and herds are brought to feed at *Los Amancaes*. During their stay, the place presents the appearance of a fair, from the number of people who go out to *picnic*, and spend the day in roaming among the hills and decking themselves with the flowers, or in dancing, horse-racing, and other sports. This annual *promenade* commences on St John's day, the *Amancaes* being then in full flower ; and from an early hour, a great part of the motley population of Lima are seen swarming towards the hills, gaily dressed in all sorts of colours, of brighter hue, but not more varied in their tints than the complexions of the wearers. When the day is fine and the mist confined to the hills, the scene is singularly picturesque. On one hand, the steep rocky sides of the valley are studded with cattle tended by their Indian owners, and gradually disappearing in the mist as they wind among the hills, the plain below, extending to the main valley of the Rimac, is covered with groupes engaged in various sports, and fresh parties constantly arriving ; while, on the opposite side of the river, with distant mountains for a background, the white spires of the city are seen through the groves of orange-trees in the gardens of the suburbs ; and lower down,

the cultivated valley leads the eye to the ocean, with the Island of San Lorenzo rising abruptly in the distance.

"The season was considered late, and the cloudy weather had not extended far inland, so that, after proceeding a few leagues, the hills were perfectly naked, and exhibited a marked contrast to the fields of maize and lucerne in the valley below. We arrived before sunset at an estate called Punchanea, five leagues from Lima. The proprietor, an old Spaniard, to whom one of my companions had rendered some essential services during the revolution, gave us a hearty welcome, and an excellent supper was prepared for us without garlic. As a compliment to our English tastes, too, the supper was no sooner removed than *tea* was placed on the table.

"Providing beds, especially to a party, forms no part of the hospitality shown to travellers in South America. Each person, if he have a luggage mule, carries bedding with him, but at all events, he has some rugs and a blanket over his saddle, which, with his *poncho*, answer the purpose exceedingly well after sitting all day on a mule. We were, therefore, of course, provided with *furniture* for an immense empty apartment, into which we were conducted, and which was to serve us for a dormitory. In Chili, most people on a journey prefer sleeping in the open air. Those who have never tasted the luxury of passing the night beneath the bright starry sky of a climate like that of Chili, cannot form an idea of the sound and refreshing sleep the traveller enjoys there, nor of the elasticity of spirits, and perfect freedom from fatigue, with which he springs from his grassy couch, when the muleteer warns him that the day is beginning to dawn, and the mules await him to pursue his journey. But in Peru, especially in the vallies near the coast, where the climate is 'fair and false,' it is usual to sleep under cover: the traveller, who, unaccustomed to the climate, should venture to pass the night in the open air, would most likely awake with an ague, and very frequently, his only alternative is to immure himself for the night amidst the smoke and filth of an Indian hut.

"June 22d.—We could not start till eight o'clock, having to wait for a fat sheep our host had ordered to be killed for us. The carcase being duly packed in its own skin, and placed between

two trunks on a mule, we set out, accompanied by our hospitable friend, who rode with us to the boundary of his estate, where he left us to pursue our journey. The road continued to wind round the foot of the hills on the south side of the valley, to the estate of Cavallero, where there is a post-house, generally made the first stage from Lima, from which it is distant six leagues. Near this place there is a bend in the valley, and in order to avoid the detour, it is usual to proceed up a ravine among the hills, from whence the road falls again into the valley, several leagues farther up. The ravine is called *Rio Seco*, (*dry river*,) and dry enough it certainly is, for not a drop of moisture is seen for a distance of five leagues, although there are unquestionable marks of its having been, at some former period, the bed of a considerable stream. This *Rio Seco* presents a very fair specimen of Peruvian barrenness, of which it is hardly possible to form an idea without witnessing it. I have already alluded to the desert appearance of the coast, where you may travel whole days, over pure sand, without any trace of vegetation; or, if the road lies occasionally near a range of mountains, the scene is only varied by masses of bare rock, of which the fragments that cover the road are as fresh and unsoiled as if they had fallen but yesterday from the hammer of a mason.

“As the day advanced, we found the heat excessive, having now exchanged the hazy atmosphere of the coast for the clear deep blue sky of a tropical mountain region. At the head of the *Rio Seco*, the road winds up a steep hill, from the summit of which, the green valley is seen at a distance of two leagues, tantalizing the thirsty traveller during the two hours that his mule takes to crawl over the rough stony bottom of the ravine that leads to it.

We regained the main valley about three o'clock, at a place called Yangas, consisting only of half a dozen houses, immediately beyond which is the village of Alcocota, five leagues from Cavallero, by the road we came; by the valley it is six leagues and a half. The valley, where we turned off, is nearly a league in breadth, but here it had contracted to about a mile, and the hills that bound it are high and steep, especially on the north side, where the rock forms a perpendicular wall.

“Alcocota is considered the boundary of the rainy district,

but the showers are only experienced occasionally ; a few leagues higher up, they fall constantly during the mountain winter.

“ At the end of two leagues, we came to Santa Rosa de Quive, a small place consisting now only of a few huts on the hill-side ; a church and some houses in the valley having been destroyed by fire during the revolution. Here we halted for the night. The little hut where we stopped was not large enough to contain one-third of our party ; but being on an eminence far above the valley, there was no danger in sleeping out of doors, and we therefore took up our quarters in an open shed.

“ June 23d.—Having been fortunate in escaping the attacks of mosquitoes, which are numerous in the valley, we rose at day-break, and started as soon as our numerous beasts could be laden and saddled.

“ Though seated on a natural platform, far above the valley, the inhabitants of Yazo are very subject to intermittent fever, and the place has a bad name, on that account, among those who travel to Pasco ; but there can be no doubt that the people bring the germ of the disease from below. They work all day in small pieces of irrigated land in the narrow valley, where the heat is increased by the reverberation of the sun's rays from the steep rocky mountains, which, at the same time, prevent a free circulation of air ; and, from the clearness of the sky, they are exposed after sunset to a sudden chill, while surrounded by a moist stagnant atmosphere in this confined situation. I was obliged to put up here on my return, and, although I had just recovered from a severe attack of the fever, I found no bad effects from sleeping in an open shed.

“ The hills near Yazo are very steep, and the road occasionally very narrow, especially in one part, where it forms a mere ledge on the side of a nearly perpendicular hill. A pass of this sort is called a *ladera*. The bank above, consisting of large rolled stones imbedded in gravel, bore evidence of the heavy rains in winter, being ploughed into numerous channels, and at that season it must be dangerous to pass. From some of the loose earth giving way during the earthquake in 1828, a man and several mules were precipitated into the valley and killed.

“ At Huarimayo, there was only a single hut, where travel-

lers usually halt their mules, previous to passing a long and elevated *ladera*, called the Pacron. This is on the north side of the stream which we had crossed, lower down, on a very frail bridge, made of branches of trees laid from bank to bank.

“On leaving Huarimayo, we continued along the north side of the valley, which is only wide enough to afford a passage to the stream. The hills rise so abruptly, that, in cutting a road, it has been necessary to carry it to a great elevation, following a natural break in the declivity of the mountain, which allowed a breadth of a few feet to be levelled between a precipice on the one hand, and the almost perpendicular wall of rock that rises on the other. The rushing noise of the stream gradually became more faint as we ascended, and died away before we reached the greatest elevation, at nearly 400 feet above its bed. The profound silence that reigns in solitary mountain-districts, where there are no trees, and we hear neither the song of birds nor the humming of insects, was only broken at intervals by the shouts of the muleteers encouraging or threatening their troop, as they wound slowly in an extended line along the sinuosities of the mountain. Sometimes, a mule, pressed by a heavy load, showed a disposition to halt and rest, and consequently stop all those in his rear, in places where it was almost impossible to reach him ; but in such cases, a mixed volley of stones and imprecations, with threats of future punishment, which experience had taught the offender not to despise, soon had the effect of putting him again *en route*.

“It chanced that several troops of laden mules, bound to Pasco, were passing the *ladera* at the same time, and followed in our rear. Our muleteers had neglected the usual precaution of sending forward a messenger to prevent others from ascending in the opposite direction till we should have descended to the valley and left the road clear ; the consequence was, that we were met at the end of the *ladera* by another party that had just ascended, and our muleteers being in fault, the others insisted that we should return ; which, however, was out of the question, as there were a great number of mules behind, in situations where it was impossible for them to turn round. After much time spent in altercation, it was agreed that as our opponents had more room

to move about in, they should endeavour to drive their mules up the hill above the road, which was there rather less steep ; this, with the assistance of our muleteers, was at length accomplished ; and while we passed down they remained perched in situations that hardly promised footing for as many goats. There is, perhaps, more risk in the descent than in the *ladera* itself, the road making several sudden turns on the face of the hill, where it is cut into steps to prevent the mules from slipping.

“ In this part of the valley, on the south side, a hill rises abruptly to an elevation of nearly a thousand feet, and from its summit, a slender stream, like a band of silver, fringed with verdure, winds its way down the steep declivity. At the highest point, though not seen from the same spot, is a small Indian village, very difficult of access, whose inhabitants are called in the *Quichua* language by the now classic name of ‘ The Children of the Mist.’

“ The valley, which had narrowed into a mere ravine, opens a little on approaching the town of Obrajillo, three leagues from Huarimayo.

“ Obrajillo is twenty-one leagues from Lima, about midway between that city and Pasco. Most of the muleteers who pass between the mines and the capital reside there ; a great convenience to travellers, as they get fresh mules for the remaining half of the journey. The valley is wide enough to allow some ground to be cultivated between the town and the stream ; and above the town, towards the south, there is a recess in the mountains, occupied by low rounded hills, which have been levelled and formed into a series of small terraces for the cultivation of grain and vegetables, a sort of work for which the ancient Peruvians were celebrated. These patches of land being irrigated by a stream of water brought from above, the tufaceous rock readily decomposes by the constant moisture, and, combined with vegetable mould washed down by the rains, forms a black fertile loam, which yields luxuriant crops without manure. On an eminence at the extremity of this cultivated land, about — feet above the level of Obrajillo, and half a mile distant, in a straight line, stands the town of Canta, the chief town of the province of the same name, and the residence of the Intendant and a Governor ; it consists, however, like Obrajillo, of small houses, little better

than huts, and the population of the two towns, chiefly Indian, is only about eight hundred souls.

“ The day after our arrival was the feast of St John, the patron saint of Obrajillo. Our muleteer being one of the *alcaldes* of the town, and named after the saint, his presence was considered necessary at the festival. It was recollected, too, that the name of my friend was likewise John, and as the people of the town are chiefly supported by the traffic occasioned by the mines, which was expected to be greatly increased by the erection of the steam engines for draining them, it was argued, that there could be no hope of patronage from the saint in the undertaking if we refused to devote a day to him. A still more powerful argument was mentioned incidentally, that, as the fresh mules were feeding at some distance, they might not arrive sufficiently early for us to start the following day, even if we wished it, which meant, as we knew by experience, that the mules certainly *would not* arrive, whatever our wishes might be, and we were therefore obliged to declare our willingness to do honour to the saint. For my part, I was very well pleased to stay, as the delay gave me time to pack up my seeds, and change the papers of my plants, and to collect a few more in the neighbourhood.

“ On the 24th, St John's day, a great part of the population of Canta and Obrajillo assembled at the church of the latter to hear mass. The walls of the building were decorated with large bouquets and festoons of flowers, and the interior was lighted up by a great number of candles, mostly furnished by the poor Indians, who are taught to consider such contributions as part of their religious duties. Among other ornaments, we were struck by a unique display of small flags, formed of coloured cotton handkerchiefs of different patterns, from the looms of Manchester and Glasgow, fastened to canes that were stuck into the walls. There was not room within the church for all those assembled, and many knelt in the open air before the door, where they remained till the mass was said; and during the whole time, six Indian girls were dancing, and singing in the Quichua language, in the midst of them. When the unfortunate aborigines of these countries were first subjugated by the Spaniards, in order to insure their attendance at the festivals of the Catholic Church, the priests allowed them to use the songs

and dances they had been accustomed to at their own feasts, and the custom is continued to the present day. After mass, the congregation, headed by the priests, marched in procession round the plaza, and while the latter were chaunting a part of the church service, the Indian girls continued to dance and sing round them, accompanied by several men dressed in the ancient costume of the Incas, some of them having their faces covered with hideous masks made from calabashes. This performance was continued even while the priests were officiating at a temporary altar, erected on one side of the plaza. Immediately after the procession, a cock-fight was exhibited, and the rest of the day was consumed in eating and drinking.

“ The next stage, of seven leagues, lies across the Cordillera, and there is no intermediate place to sleep at.

“ A little below Culluay, the valley expands. The alcalde, at whose dwelling, being the largest in the place, we intended to pass the night, was absent, like the rest, in the fields; and the door being locked, we had to wait patiently for his return.

“ It was evening when we arrived, and, as the sun went down, we found the air piercingly cold, so that we were glad to crowd round the turf fire of a smoky hovel, that served for a kitchen, till our host came, and supper was prepared. A party from Pasco arrived shortly after us, and informed us that snow had fallen for several days in some parts of the Cordillera, where partial snow-storms are occasionally experienced at all seasons, but it seldom falls till after midday.

“ 27th.—As we had a rough day's journey before us, we were obliged to rise before daylight, and several of our party who had not quitted Lima for some years, did not at all relish the fresh morning air, at a temperature very little above freezing.

“ Advancing from Culluay, there is a great sameness in the appearance of the hills, which are mostly covered with a smooth compact turf. A long *ladera* conducted us to the head of the valley, where we crossed a small rill that takes its rise in a neighbouring ravine, and is the source of the river of which we had traced the course from the coast. At this spot, called the Alto de Jacaybamba, two leagues from Culluay, we commenced the ascent of the Cordillera. It was steep and rugged, lying among rocks of porphyry and *tufa*, like that at Canta. A large

insulated hill, called La Viuda, several hundred feet above the road at the highest part of the pass to which it gives its name, consists of the latter rock, enclosing large rolled masses of porphyry.

“ On gaining the summit, a league from the bottom of the mountain, we came upon a more open country than we had seen since the commencement of our journey, though the prospect was still somewhat limited, the lowest situation being, of course, chosen, for the road, from which we only had a partial view of the higher mountains in the distance, covered with perpetual snow. We descended very gradually among low ranges of black limestone, with impressions of shells; a very extensive formation in this part of the Andes. The skirts of the hills, and the little vallies and tracts of level ground between them, were covered with verdure, and the deeper hollows were occupied by lakes, the water of which was of a dark blue colour, more intense than that of the waters of the ocean; in some of them, the deeper tint was varied by patches of bright turquoise, from the bottom being of a whitish colour, and the water less deep.

“ The snow that had lately fallen had already disappeared from the open ground, but much of it still remained in sheltered places.

“ Before leaving Culluay in the morning, we had been advised not to take any breakfast except a little broth, to lessen the risk of suffering from the *puna* or *veta*, the sickness that frequently attacks those who are unaccustomed to breathe the rarified atmosphere of these elevated regions. None of the party, however, had experienced any inconvenience in the ascent; and after proceeding about half a league, where the elevation was considerably less than at the pass, we halted at one of the lakes, and breakfasted on some cold meat. But we soon found that we had calculated rather hastily on our exemption from the *puna*; and that, like sea-sickness, which it resembles so much in other respects, it does not always immediately attack those who are liable to be affected by it. Before we had advanced half a mile from the lake, several of the party began to suffer from head-ach; and Mr M—— and myself were so ill that we found some difficulty in reaching the end of the stage, a place called Casa-cancha, to which we descended about four o'clock. We

had scarcely arrived when we were both seized with violent vomiting, accompanied by all the sensations that usually attend sea-sickness, but the throbbing pain of the head was much more acute and depressing.

“ It is generally said that difficulty of breathing is one of the most common and distressing symptoms of this disorder, but it is not the case. Few people suffer in their respiration, although the pulse is frequently accelerated, while they are riding or walking on a level road; it is only in walking over rough ground, or in climbing the mountains, that respiration is affected, and it then becomes necessary to halt repeatedly and take breath. The miners work as hard, and perform the same quantum of labour at Pasco, fourteen thousand feet above the level of the sea, as those at a sixth-part of the elevation; and indeed the English mechanics there, unless very laboriously employed, performed their work without suffering any inconvenience from this cause. Several of our party, who had frequently been at Pasco, and resided there a considerable time, were not affected; and, indeed, the only one who was attacked besides Mr M—— and myself, recovered almost immediately. In this, too, the *puna* resembles sea-sickness, that different individuals, under precisely similar circumstances, are affected in a very different degree, and many do not suffer at all. It is, however, worthy of remark, that the same persons are not equally affected by the two disorders. My friend and I, some years before, had made a voyage of four months together, and he only suffered for a few hours from sea-sickness, while I was never wholly free from it during the voyage; but, in the present instance, he suffered more severely than I did. Persons of full habit, affected with the *puna*, frequently spit blood. Some months before we passed, an Englishman, who had been employed at the mines, set out to walk to the coast; he had previously been in bad health, and shortly after his arrival at Casa-cancha he died, from hæmorrhage, having burst a blood-vessel in the lungs.

The valley of Casa-cancha is about half a mile wide, abruptly terminated by the limestone hills we had passed over, and bounded at the side by red sandstone and conglomerate. We were lodged at a miserable hut, built of stones and mud. The single apartment, of which it consisted, served us successively for

a kitchen, dining-room, and bed-room ; the dinner was cooked over a turf fire, the smoke from which eddied round the roof, and then partially escaped by the door-way, which was only about four feet high.

“ At daybreak, on the 28th, when we raised the piece of sooty cloth that served for a door to our dwelling, the whole valley was so thickly covered with hoar-frost, that it appeared as if snow had fallen in the night. We were delayed more than two hours from some of our mules having strayed to the hills. As the beasts are suffered to wander about at night in search of pasture, an Indian is generally to be found where travellers halt, who, for a trifling reward, undertakes to keep them together : he passes the night in the open air, frequently in the midst of frost and snow, and is seldom known to sleep, or neglect his charge. In this instance, a dense mist had filled the valley, and no blame could attach to the watchman ; but from the abuse that was lavished upon him, one would have thought that he had been taken in the act of stealing all our mules, instead of merely not having seen one or two of them through the mist on a dark night.

“ Mr M—— continued very ill, and although I felt tolerably well when I rose, we had not travelled half a league when the headach returned with increased violence, and it was greatly aggravated by the motion of the mule. Some of our companions, too, were similarly affected in the course of this day's ride.

“ The valley, and even the slopes of the hills, were so swampy, that we travelled very slowly for about a league and a half, when we left the valley, and crossed over some high land that brought us to the small river of Palcamayo, three leagues from Casacancha. From Palcamayo, we passed over a succession of low hills, covered with short grass ; the intervening hollows were very swampy, and we were frequently obliged to make a long circuit to avoid them. The whole of the district through which we were travelling, including the plain of Bourbon, whither we were bound, and the country for many leagues to the north and south, may be considered as forming an immense basin between two distant Cordilleras ; and from the quantity of rain and snow that falls in winter, and the thawing of the latter on the summits of the hills and sides of the mountains during summer, the

subsoil in the low grounds is always saturated with moisture, and numerous springs issue from the surface. On the plains and slopes of the hills, there is, all the year, an abundance of grass, which serves to pasture large flocks of sheep for the supply of the vallies and the towns on the coast.

“Owing to the badness of the road, our progress had been so slow that it was long after dark when we arrived at a ravine, down which we continued to Huayllay, a small Indian town, and the centre of a mining district, eight leagues from Casa-cancha. The monotonous appearance of the hills among which we had travelled, at a very slow pace, the intolerable headach we suffered, and the benumbing cold of the evening wind, made this altogether a fatiguing and unpleasant day's journey.

“One of our party, a Spaniard, conducted us to the house of the Governor, who was his countryman. He had been a soldier in the Spanish army, but having married an Indian woman of Huayllay, he settled in the town, where his intelligence and activity procured him the office of Governor; to which he added the profession of a miner, and the trade of a shopkeeper. Our apartment was in keeping with the mixed pursuits of the master of the house; the table was covered with papers relative to the number of recruits, and the tribute to be furnished by the Indians under his jurisdiction; a heap of silver ore occupied a corner of the mud floor, and candles, sugar, jars of spirits, and similar merchandise were spread around, with very little regard to arrangement. Our host and his dark lady vied with each other in ministering to our wants; and, stretched on the floor of their domicile, we soon forgot the *puna* and our tedious ride from Casa-cancha.

“In the morning, we were again greeted by the glittering hoar-frost, which added to the desolate and wintry aspect of the town and surrounding hills, where not a tree nor a trace of cultivation was to be seen. All the corn and vegetables consumed by the inhabitants are brought up from the vallies, and have to be carried fifteen or twenty leagues.

“Having arrived at Huayllay at night, we had not perceived that the valley in which it stands passed through an extensive formation of *trachyte*. A small stream runs in the bottom, from which a steep grassy slope rises on each side, surmounted

by a craggy and precipitous wall of the *trachyte* rocks. Half a league below the town, we passed some hot springs: the water leaves a calcareous deposit, which is gradually encroaching upon one side of the valley.

“ At the lower extremity of the valley, the *trachyte* is divided into columns, coated with blackish *lichen*, that from a distance gives them the appearance of *basalt*. The masses have been very differently acted upon by the weather, some parts disintegrating more readily than others. Many columns stand alone; they are obliquely divided by transverse fissures, and the different joints have been unequally worn, so that a slender shaft sometimes supports an immense mass on its summit, and the whole has the appearance of architectural ruins, interspersed with grotesque colossal figures.

“ On emerging from the valley, we found ourselves at last on the plains of Bourbon, which extend fifteen or twenty leagues from north to south, presenting a surface of green sward as level as a bowling-green. We had now only to travel six leagues to the mines, situated among the hills on the opposite side of the plain. At the distance of two leagues, we passed a low belt of limestone, and we crossed three small rivers on our way. When we looked at the vast meadow over which we were travelling, bounded by gently swelling hills that shut out the view of the distant snowy peaks, we could hardly persuade ourselves that we were fourteen thousand feet above the level of the sea.

“ Having reached the further side of the plain, and crossed a range of limestone rocks, we came abruptly upon the town and mines of the Cerro de Pasco, which occupy one side of an open space, about half a league across, and nearly surrounded by rugged hills. The satisfaction we felt at having arrived at the end of our journey was increased upon entering a house fitted up by the Pasco Peruvian Company, and finding ourselves in a comfortable apartment, with boarded floor and glass windows, and a coal fire blazing in an English grate. Mr M—— was the only one who continued to suffer from the *puna* after our arrival; he was seriously ill for some days, and confined to his room upwards of a week.

“ This celebrated spot, from which so much wealth has issued, has a wretched appearance; the town consists of narrow

straggling lanes, the houses are small and dark, and the mass of the people squalid and miserable. Heaps of refuse from the mines surround the town, which is built immediately over some of them, and there are many shafts opening into the public roads without any fence or covering, so that, on a dark night, it is impossible to pass from one part of the town to another without imminent risk.

“ I was struck here, as at Huayllay, by the gloomy aspect of the scene, notwithstanding the sun was shining in the midst of a bright sky ; but I soon perceived that the very clearness of the sky was the cause of this phenomenon. From the perfect transparency of the atmosphere, the sun's rays are unimpeded, and a dazzling light falls on objects directly exposed to their influence, while those that are in the shade receive very little indirect light from the dark blue heavens ; and thus, a striking contrast is produced between the glare of the illuminated surfaces and the unrelieved shade, very different from the effect of the diffused light in the hazy atmosphere of the coast. This gloomy effect is increased by the total absence of trees, and the general prevalence of sombre hues in the surrounding objects : the hills of bluish limestone, with the meagre unhealthy vegetation thinly scattered upon them ; the stagnant lakes, buried in their recesses, and reflecting their darkened images ; the decayed thatch of the houses ; the grey and brown dresses of the Indians, whose dark faces are mottled with purple blotches from constant exposure to the bleak atmosphere of the Cordillera, all conspire to increase the sadness of the scene, which looks as if a neutral tint were passed over the whole landscape. But all this is amply compensated by the beauty of the nights in fine weather, especially on the hills above the town, which command a view of the snowy ranges that surround the plains of Bourbon ; the colour of the sky is deep indigo, and the stars shine with a splendour that can be but faintly conceived by those who have witnessed their brilliance on a clear frosty night in Britain.

“ While in Pasco, I received several letters from Dr Pæppig, who crossed the Cordillera about a fortnight before us. He had suffered severely from the *puna*, and continued dangerously ill during the few days he remained in Pasco, from whence he

passed on through Huanuco, to Pampayaca, the most remote estate on the river Hualluga. Previous to leaving Chili, this accomplished and enterprising naturalist had spent eight months in exploring the interior of the province of Concepcion, and had examined the Flora of an extensive district in the mountains adjacent to the volcano of Antujo, where no botanist had ever before penetrated. Dr Poeppig's Chilian collections are among the most extensive and valuable ever made in South America. In Peru, his intention was to examine the productions of the country bordering on the river Hualluga, and then to endeavour to descend by that stream to the Amazons, and return by way of Para to Europe.

“ On the 24th of July, I made an excursion for a single day to Huayllay ; and, on the 28th of August, I set out on my return to Lima, where I arrived on the 2d of September.”

ART. XIV.—*Account of the Russian Steppes, with Geognostical Observations made in a Journey to the Environs of Mont Elbrouz, in the Caucasus, undertaken in 1829 by order of the Emperor of Russia.* By M. KUPFFER of the Imperial Academy of Sciences.

TOWARDS the north the chain of Caucasus gradually descends into the immense Steppe which constitutes the middle of Russia. From Voronetz to Stavropol we scarcely meet a single hill or tree. Houses become more and more rare, and are collected on a small number of points where the action of some river interrupts the uniformity of the soil, and renders more fertile the black soil of the steppe, which is sterile by excess of strength, by diluting it with sand. In passing through these villages, we are equally surprised with the sight of a few trees, and sometimes we see in the distance sites crowned with wood. In these countries it would appear that the inequality of the soil is a condition necessary to the formation of forests; the cause of which probably is, that the rocks which have never been overturned, present too few discontinuities to allow a sufficiently free passage to the waters necessary for the support of a considerable vegetable mass.

We also travelled from Taganrog to Nicolaieff, the steppes which border on the Black Sea and the Sea of Azoff. These steppes present the same character of uniformity and sterility. In July, everything is burnt up and yellow by the heat of the sun, whose rays meet with no obstacle;—we find ourselves in the middle of a desert; notwithstanding the rapidity of our course, (the roads made by nature are excellent, and we run no risk in riding with a loose rein, for we meet with no inequality, nor a stone nor a ditch,) we scarcely saw within our view more than a dozen huts of Cossacks, who have established themselves in the vicinity of some muddy spring to furnish post-horses for travellers.

In order to form a correct idea of the nature of a steppe, we must visit it in spring, when the plants which cover it are not yet withered, or rather destroyed by the heat of summer. I had occasion in the spring of 1828 to see the steppes which surround Serguiewsk, in the north of the government of Orenbourg, and I shall here describe it as a substitute for that of the steppes of the Black Sea, for which I was unable to collect the necessary materials, because we visited it when the season was too far advanced.

Serguiewsk is celebrated in eastern Russia on account of its sulphureous springs, which attract every year crowds of invalids. This establishment, if we may give this name to thirty cottages neatly built, and inhabited only in the fine season, is situated at the confluence of the Sourgout and the Soka, $1\frac{1}{2}^{\circ}$ nearly to the east of Kasan, and in lat. 54° . It is at the foot of a hill composed of calcareous rock mixed with gypsum, that the mineral springs rise, containing sulphuretted hydrogen and carbonic acid. When we are on the summit of this hill, we see the land rise insensibly, and terminate by forming an immense plateau, where the eye loses itself in the horizon without meeting with any elevation. Such a plateau presents the character of a true steppe.

The physical character of the steppes consists in great dryness, and in very considerable variations in the temperature of the air. Their dryness is easily explained by their relative elevation and by their uniformity of level: It would appear that a certain inequality in the ground is an indispensable con-

dition to the formation of springs; and it is this inequality, indeed, which can alone produce the necessary pressure for causing the water to rise above the surface of the earth. It is more difficult to explain the difference between the climate of the steppes and that of unequal ground, but it is a fact which is well established. The changes of temperature are greater in the steppes, the winter being colder and the summer warmer. Russian travellers who have lately visited the steppes which separate the Sea of Aral from the Caspian have been almost always exposed to a cold of from -20° to -25° of Reaumur, (-13° to -25° of Fahrenheit,) accompanied with a very high wind. The north and north-east winds, which blow violently during winter, meeting with no obstacle, carry into the steppes the climate of the colder regions from which they come; whilst in summer the south and south-west accumulate as it were there the heat with which they are charged, and which is neither absorbed by evaporation nor extinguished by any shadow. During the day the atmosphere is insufferably hot; during the night the variation is so strong that a very sharp cold is sometimes experienced. These considerable and sudden variations of temperature destroy the tender vegetation of annual plants, and leave only the perennial ones, whose ligneous and hardy stalks offer a greater resistance to their influence. The plants which are numerous are the *Robinia frutescens*, the *Hedysarum grandiflorum*, the *Astragalus Austria-cus* and *sulcatus*, the *Oxytropis caudata*, and *pilosa*, and other papilionaceous plants; several species of *Artemisia*, and the *Prunus cerasus nana*, the cherry tree of the steppes, which scarcely reaches a foot in height, and whose sour fruit is collected by the inhabitants. Wherever the steppe is traversed by rivers, or where a slight inequality in the ground permits the water to amass in a greater quantity, the hardy plants soon disappear beneath the plough, and the steppe is changed into a productive soil; trees multiply on the heights, and houses and villages appear.

It is thus that the banks of the Don, and of the Donetz, (Little Don) and other rivers which flow through the country of the Cossacks, are peopled. A great number of villages, some of which are very considerable, stretch along the banks

of the Don; but in receding from the river we find ourselves in the middle of a steppe and in an inhabited country. The Don near its embouchure at Tcherkask widens considerably as it recedes from the hills composed of a chalky calcareous rock, which present to it on its right bank an insurmountable barrier; it inundates every spring a great extent of country, and spreads over it abundance: From the Axais to the Tcherkask, the culture of the vine, fisheries, agriculture, and commerce, give employment in their turn to thousands of men, who draw from them a certain and an agreeable subsistence. This is one of the most animated scenes that I have ever seen, and particularly strikes the imagination of the traveller who has been travelling over the steppe.

Several hills which advance towards the Sea of Azoff, on the right bank of the Don, rise nearly to the height of 500 feet above the level of the Don, and are composed of a calcareous rock filled with fragments of shells, which it would be difficult to refer to their species. New Tcherkask is built on one of these hills: This town is as it were placed on a promontory, which advances from the steppe of which it forms the prolongation towards the Sea of Azoff. Its position partakes in the dryness and sudden changes of temperature of the adjoining steppe, without sharing in the advantages which the Don sheds over its banks. The inhabitants are obliged to collect the rain water in cisterns, in order that they may be supplied in the heat of summer. The town has little trade, and its existence depends chiefly on its being the centre of government, and the chief place of the Cossack territory. The calcareous rock which forms the soil has a wide extent. We meet with it every where from Stavropol to Nicolaieff, and it forms the shore of the Black Sea. Sometimes it is filled with broken shells, and sometimes it is formed in beds more or less destitute of organic remains. Though this rock most frequently presents only a heap of the debris of shells cemented by a calcareous paste, we sometimes recognize entire specimens, particularly the *Cythereæ*, sometimes the *Mytilites*, and generally sea shells. This rock is always arranged in horizontal beds. It is only beyond Stavropol, and in advancing towards the mountains of the Caucasus, on the road to the hot springs, that

we see a change in the aspect of this rock and of its contained fossils: Sometimes it is very compact and of a grey colour, whereas the rock containing the Cythereæ, when devoid of shells, is always very white, and generally so soft that it is often friable: Sometimes it becomes a little more porous, and contains some fossils of the family of the Helices. It is with this limestone rock that the country begins to present an undulated surface, so that we perceive that the mountains are approaching. In quitting Stavropol, we descend into a wide plain, at the extremity of which is situated the village of Nadeja, twenty versts from Stavropol. This limestone (which I shall call the helical limestone, to distinguish it from the cythereal limestone,) extends to Kalinova; and it is between this village and the small town of Alexandrovsk that we observe on the right, in descending a hill, some insulated rocks of sandstone, and, at the same time, the snowy top of Elbrouz (or Chad,) and a part of the central chain of Caucasus presents itself for the first time.

This sandstone is distinguished by a particular bivalve shell, with which it is filled, and of which M. Pander (who has thrice visited the shores of the Black Sea,) has formed the new genus *Hypania*. The sandstone is of a yellow colour, it slightly resists the blow of a hammer, and sometimes becomes even friable; it is liable to perpendicular cleavages, and to form enormous blocks with salient angles, and of a grotesque form; it effervesces strongly with acids, even at places which contain no fossils; and there are grains of sand enveloped in a calcareous mass, which is sometimes deposited in separate beds.

A phenomenon deserving of notice is the great fertility of this soil. It appears that the sandstone, being more porous than the calcareous rock, the subterranean waters circulate in it with more facility, and distribute themselves more uniformly, so that plants whose roots are almost constantly moistened resist better the burning heat of the sun in the steppes. At some points the country loses entirely the character of a steppe. We meet with trees, cultivated fields and lakes, habitations multiply, the means of subsistence increase, whereas everything in the steppe languishes.

Behind Alexandrovsk, the view of the chain of the Caucasus

is more and more developed : It occupies towards the south east a great part of the horizon. It commences on the right with Elbrouz, where two summits rise high above the rest of the other mountains, and it terminates on the left with Kasbeck. This chain is more visible, as we advance to near the mineral springs into a plain whose undulating surface rises nowhere so high as to intercept the view of the mountains. We observe, however, even here several insulated cones which rise in front of the snowy crest, and whose sombre colour may be distinguished from it. These are Bechtav and the other trachytic hills which surround the hot springs. At Sablinsky, and at Gheorgievsk, their aspect becomes more and more imposing : In quitting this last town, the capital of the Cossacks of the line, we enjoy a very agreeable, and at the same time a very instructive view, for we are struck with the relation between the forms of these trachytic cones and the central crest of Caucasus, whilst the chain of mountains which separate them, and whose height is much greater than that of Bechtav, presents a series of elevations whose undulating outlines form a strong contrast with the precipitous rocks of which the central chain is composed.

Before reaching the hot springs the formations are observed to change : The mineral waters themselves rise from a compact calcareous rock : The Mahouca, a mountain 1000 feet above the hot springs, and at the foot of which they have their origin, is entirely composed of this rock : Fossils are rarely found in this limestone : Ammonites and Terebratulites have, however, been discovered. I shall call it the Ammonite limestone, because this kind of fossil particularly characterizes it.

The hot springs which abound with carbonate of lime have deposited a particular sediment filled with impressions of leaves : This deposit is in some places considerably thick. It covers entirely the foot of Mahouca, and rises here and there along the flanks of this mountain, where it sometimes forms small caverns. It is easily distinguished from the limestone of which the Mahouca consists, because it is deposited in the manner of sediment in thin and undulated strata. It is some-

times susceptible of a high polish, and often presents very agreeable delineations.

The most remarkable rock in the vicinity of the hot springs is that which composes the Bechtav. This mountain forms a mass perfectly insulated, rising rapidly 3000 feet above the plain, and 4000 above the Black Sea. Its five conical summits have given it the name of Bechtav, which signifies *five mountains* in the Nogay language. The rock of which it consists is a white trachyte, with small crystals of glassy feldspar, some grains of quartz, and small needles of green amphibole disseminated through a white paste of compact feldspar. The strata of calcareous rock which rest on the foot of this mountain do not appear to have experienced any ulterior change in their primitive position, which proves that they have been deposited since the eruption of the trachytic masses.

The hot sulphureous springs are several versts distant from Bechtav; but we find at its base ferruginous springs, which are likewise hot or lukewarm. In the vicinity of Bechtav there exist other mountains which resemble it in their external form and composition, for example, Verblioud, (Camel,) the Lissigora, (mountains of foxes,) the Zmeievaia, (mountains of serpents.) These mountains are insulated, and are not even placed in the same line.

In advancing from the hot springs to the chain of Caucasus, we do not quit the calcareous country till we descend on the banks of the Malka. We at first see rising on the opposite bank of the river mountains of a particular form: we find upon their declivities immense and angular blocks, which we had previously found above sandstone with hypanites. The strata themselves, though horizontal, present a great number of vertical fissures, and the rocks which they compose are very rugged. It is a sandstone characterized by the Ostracites which it contains. The Malka separates for a long time this formation from the calcareous formation of which we have spoken. In our route from the stone bridge over the Malka at Kinjal, we crossed the sandstone country in a direction from N. to S. It here rises to the height of 5000 or 6000 feet above the sea, and forms plateaus of considerable extent, intersected in every direction by crevices and deep grooves, where the

horizontal stratification of this rock appears. Upon this hill, not far from Mahomet Kourgan, whence we discover the valley of Baksan on the Haimacha, we at last see the sandstone rise with a calcareous rock on which it rests, and which is the oldest secondary limestone of the Caucasus. In the valleys of Kichmalka and of Kassaout we have met with a sandstone, which, in the series of superpositions, ought to be the same as the sandstone with ostracites; it is, however, entirely devoid of fossils. In the valley of Kassaout, it is very compact at places on a lower level, where it sometimes contains strata of argillaceous schistus. It is very quartzzy at the same place, and does not effervesce with acids, while on the heights it is much mixed with carbonate of lime.

In an excursion in which we went out of the valley of Kassaout, in advancing towards the central chains to seek for a lead mine which the Tsherkesses had mentioned to us, we met on the heights which we traversed a very coarse conglomerate; rounded pebbles of common white quartz, grey brown and black jaspers, and some fragments of argillaceous schistus, are all imbedded in a quartzzy cement.

In spite of the most careful examination, I could not find any fragment of trachyte in this conglomerate.

When we have reached the boundary of this plateau, we are still separated from the central chain by a wide and deep valley, and it is while descending a little towards this valley that we suddenly meet with nearly vertical strata of a particular rock, against which the above-mentioned conglomerate appeared to lean. We soon discover ancient works for obtaining the particles of sulphuretted lead disseminated in the rock. In the parts near the conglomerate this rock is also composed of pieces of rolled quartz, cemented by a very ferruginous clay, (not far from this is found much ochre of yellow iron,) in other places the relative quantity of the pieces of quartz diminishes, the paste predominates, and we see it distinctly composed of two substances, feldspar and sulphate of barytes; it is also the sulphate of barytes which forms the gangue of the sulphuret of lead, and the thin strata of it are traversed by narrow veins of the compact sulphurets of lead and antimony, grey copper,

and oxide of manganese, all of which are so small in quantity that the works were soon abandoned.

It was in our excursion to Kinjal and to the Ourda that I had occasion to study the secondary formations which were older than the sandstone with ostracites. We have already seen that this sandstone rests immediately on a calcareous rock in which I have not met with any fossil remains. On the Haimacha, where we halted the 10th of July, this limestone, though generally compact, had small lamellæ of carbonate of lime disseminated through it. The rocks which follow the banks of the lower Ourda are composed of this calcareous formation, which appears to have a great extent, as we shall presently see. I shall call it limestone with gryphites, for I afterwards met with very large and beautiful specimens of this fossil in a limestone rock which I believe belongs to the same formation. I have shown in the historical part that the Inal, the Kinjal, and the Bermamuc, compose a particular chain of mountains, the highest of the secondary mountains, and the nearest to the central chain. These mountains form, so to speak, the rugged edges of a long and wide crevice, from the bottom of which rises the trachytic chain, against which the secondary formations which I have described would appear to lean, if they were not separated by deep vallies, where they are seen to perforate several intermediate rocks.

The Kinjal and the Bermamuc, which we have examined closely, are composed of this grey and compact calcareous rock which ought to be referred to the limestone with gryphites, though in those same points I have not met with those fossils which in lower points, and in precipices, turned to the central chain, are mixed with sand, and become at last a true sandstone, still older than the sandstone with ostracites. These two formations rise to the height of 7000 or 8000 feet above the level of the ocean.

It was in descending towards the upper Ourda, which here flows in a deep crevice, that I met with the first intermediary rocks, and at the same time with lava and amphibolic rocks.

The rocks hitherto mentioned are disposed in horizontal beds, which rise almost insensibly towards the central chain; but in the disposition of the intermediary rocks a much greater

disorder is exhibited. We soon discover the cause of these derangements. Scarcely has the limestone with gryphites begun in the lower beds to mix itself with sand, and to change into sandstone, when we see it elevated and torn at several points by black and red lavas, and by compact trachytic masses. We descended in zig-zag the almost perpendicular declivity of this mountain; and when we arrived below, we saw to the bottom of a precipice, or of a crevice, on the banks of the Ourda, which falls impetuously from rock to rock. Here we discover heaved up strata of argillaceous schistus.

The lavas which are thus found at the limit of the sandstone and of the argillaceous schistus are mostly of a black colour, filled with vesicles greatly elongated in one direction, and lying parallel to one another, so that these lavas seem to have suffered a great pressure by the superposed beds in traversing them. These vesicles are entirely empty; and with the exception of some white points nearly microscopic, we cannot discover any trace of the substances which so often fill the cavities of other lavas. Sometimes small crystals of amphibole are enveloped in their paste. At other places the cavities are very rare and very small. The mass becomes highly compact, takes a gray colour, mixed with small spots of a pale red, cleaves into thin laminæ, and presents in general the mineralogical characters of a trachytic paste: The crystals of glassy feldspar are, however, wanting; and we discover here and there small brilliant points of mica of a bronze colour.

The argillaceous schistus alternates in some places with a species of psammite, (*grauwacke*,) which is, however, distinguished from the common *grauwacke* by its white colour. The strata of this rock are very much inclined; on the banks of the river Kinjal, which we descended on the same day, the beds of argillaceous schistus present so many irregularities in their arrangement, that it is easy to observe the effect of a general derangement occasioned by the eruption of trachytic masses, which, as we shall presently see, penetrate every where the soil of the argillaceous schistus.

The interval between the last sandstone which I have mentioned and the argillaceous schistus, is occupied with a particular formation of *gres houiller*. This I particularly observ-

ed on the height of Kharbis. This height, which receives its name because this plateau, which forms only a hollow surrounded with higher mountains, rises a few hundred feet above the banks of the river Kharbis, forms, where it advances to the river, and at the same time where it advances towards the central chain, that is, towards the south, a precipice where the strata of which it is composed are laid bare. We see distinct alternations of beds of coal some inches thick, with a quartz and yellowish sandstone. At some distance to the south, before the eye arrives at the central chain, or rather at Elbrouz itself, which appears from this in all its grandeur, there is seen rising to a very considerable height a very steep mountain, with a rugged crest, whose flanks are covered with rocky debris. This mountain has the same physiognomy as the trachytic mountains which compose the central chain. It forms part of a particular chain which runs parallel to the central chain, and which we must ascend in approaching the latter. These mountains, which touch the limit of perpetual snow, are composed of a dioritic rock, whose character is so remarkable as to merit a special description.

After having gone along several precipices which display alternately horizontal beds of sandstone and coal, of which all the district is composed, and whose rugged walls are turned to the central chain, we descend rapidly into a small hollow which separates the Youngouché, (this is the name of the mountain,) from the plateau which surrounds it towards the north. We ascend again very rapidly, in order to reach a height which leans to the south on the Youngouché, and forms, as it were, the first step of it. This height is composed of a sandstone similar in external characters to that which contains the coal strata, but which differs from it considerably in the disposition of its beds, which are highly inclined, and almost perpendicular. These strata are supported against rugged rocks, with which the crest of Youngouché is flanked at its base, and which consist of an argillaceous schistus, and of a conglomerate formed of fragments of quartz and blackish gray jasper (Lydian stone) cemented by a talcose substance of a pale yellow-green colour, unctuous to the touch, and of a compound and schistose texture. At some points this conglomerate takes the appearance

of psammite, to which formation it should doubtless be referred. The slope of the mountain is on account of its declivity so covered with debris that it is very difficult to penetrate to the rock. Between these debris, beside the rock which forms the summit of the mountain, and of which I have just spoken, and the conglomerate also mentioned, pieces of serpentine and schistoidal amphibole.

The Youngouché itself, that is, the crest of this mountain, is almost inaccessible. The ascent of it is dangerous; the irregular debris with which its flanks are covered slide beneath the feet, and tumble into the abyss. The rock of which this mountain is composed is a diorite, in which compact feldspar predominates; it is a grayish mass containing small needles of amphibole, which sometimes unite into nodules and fill the interior of small cavities. This grayish mass seems to be an intimate mixture of feldspar and amphibole; the feldspar presents here and there points crystallized and whiter than the rest, so that one is sometimes tempted to take this rock for a porphyry. Microscopic points of sulphuret of iron are sometimes discovered in it.

The conglomerates which I have described appear also in various other points situated on the boundary of the trachytic and secondary district, for example near the cascade of Touslouchapap. The cascade itself is formed of a similar and very large conglomerate, which rises perpendicularly above the banks of the Malka to a considerable height. Enormous blocks, which are no doubt detached from these rocks, obstruct the bed of this river. On a slope less steep, upon which our camp was established, we saw some rocks of serpentine and of intermediary limestone penetrated by a stratum of the vegetable soil which covered it.

The stone bridge of Kouhun is also formed of porphyroidal diorite; the dioritic mass is of a pale green colour, and contains white crystals of feldspar. These amphibolic rocks form here, as they do every where in Caucasus, very rugged mountains, bristled with aiguilles and crests, whose strange aspect and sombre colour contrast singularly with the flat but rugged rocks of sandstone which follow in the order of their superposition.

We have seen that the amphibolic and dioritic rocks form a series of very considerable elevations, intermediate between the central chain and the chain of the more ancient secondary rocks, of which the Kinjal, the Inal, and the Bermamuc, form a part. These rocks seem to have been produced by an eruption which has taken place in the region of the argillaceous schistus; for whenever they appear, we meet also with masses of this last rock heaved up in the most regular manner, and evidently broken and altered by their contact with the dioritic rocks. The schistose amphibole, particularly, is so confounded with the argillaceous schistus, that it is difficult to distinguish these two rocks, which sometimes alternate in very thin strata, and which have the same colour, and almost the same texture and fusibility. It is almost worthy of remark, that we meet almost every where in the line of this soil, (the dioritic soil does not occupy a great extent,) with springs of acidulous water containing oxide of iron and carbonate of lime. The Touslookchapap derives its name from a spring of this kind which rises at the foot of the cascade. The acidulous spring near Yesilkol rises out of the ground like that of Youngouché which I have described. This spring deposits so great a quantity of carbonate of lime that the whole declivity of the mountain is covered with it. The taste of the water is very styptic, on account of the oxide of iron which it holds in solution.

The central chain of the Caucasus, which rises considerably above the mountains hitherto described, is entirely composed of trachyte. At a distance it exhibits a series of rugged rocks, crests, and peaks, whose black summits break in a singular manner the masses of snow which fill their crevices. These crests rise to the height of 12,000 feet. The perpetual snow begins in the Caucasus at the height of 10,000 feet.

The Elbrouz rises in the middle of these angular masses in the form of a double-topped cone. The smoothness of its outline would distinguish it from the surrounding summits, even if it did not rise so much above them. Its height is 15,400. Its advantageous position still adds to the impression which the view of it makes upon the spectator, for it occupies the most advanced corner of an angle which the chain of the Caucasus forms in this place, as may be seen in the maps. The

top of Elbrouz is entirely covered with snow, which is easily kept on its sides, which are generally not very steep.

When we arrive near the middle of its declivity, we perceive that the lowest trachytic crests which surround it range themselves around it, and encircle its base. Elbrouz itself seems to rise in the middle of a cavity, whose margin is formed by the trachytic crests I have mentioned. In looking northward we see the Kinjal, the Inal, and the Bermamuc, present the same arrangement. All these mountains exhibit precipices turned towards Elbrouz, or towards the central chain; while to the north they slope insensibly, and form the immense plateau of which we have already spoken.

The trachyte, (*i. e.* all the porphyries which envelope in their mass crystals of vitreous feldspar,) which compose the summit of Elbrouz itself, holds a middle place between the resinite feldspar and the obsidian porphyries. Its mass, which envelopes many white crystals of glassy feldspar of an average diameter of two or three lines, is black and opaque, of a rough and uneven fracture, and of a vitreous aspect. On the fissures which traverse it in all directions, and which divide it into masses of a form approaching to that of the parallelipiped, it is coloured red, probably by the oxidation of the oxide of iron which it contains. Small scales of black amphibole and of black or bronzed mica are disseminated through the mass.

The approaches of Elbrouz present the picture of destruction. Enormous blocks of the trachyte which I have described are heaped one upon another. Their debris cover the bottom of the excavations, and of the high valleys which separate them. These debris often preserve, while decomposing, their salient edges, and prevent the formation of vegetable soil. Though traversed by a thousand brooks fed by the melting of the snow, the ground is parched, and rolls down with a noise beneath the feet of the traveller. The rocks which I collected during this ascent present different varieties of trachyte and lavas; they are disposed without order, and they obstruct the beds of the rivers, particularly that of the Malka, from which I have obtained a great number of them.

Among these we particularly distinguished a lava of very porous and grey feldspar, which envelopes white crystals of

glassy feldspar, and small scales of black mica. This lava resembles much the trachyte of Elbrouz in its composition, differing from it only in the porosity of its mass. The cavities or vesicles are elongated in a determinate direction, and arranged in strata, a very distinctive character, which seems to belong to all these melted masses. This character sometimes vanishes; the mass becomes more compact, and its colour more sombre. The rock is thus a true trachyte with a base of obsidian, which perfectly resembles that of the summit of Elbrouz. Other specimens have a pale violet colour, with the aspect of wacke, enveloping also crystals of glassy feldspar and scales of mica. I have also met with specimens in which very thin strata of black trachyte, with a base of obsidian, alternate with a red and very porous scoria.

The porous feldspar lavas with crystals of glassy feldspar, which I have mentioned, rarely envelope portions of hyaline quartz. These portions are very transparent, and often cracked, so that they separate into several grains when touched with the point of a knife. These grains, however, preserve the ordinary hardness of quartz. These gray lava sometimes become so porous, that they form very light masses, decomposed at the surface, white and similar to pumice-stone, which they resemble also in roughness. The debris of this rock forms accumulations, which we have several times observed in returning from Elbrouz.

There still remains to be described a particular rock which I have frequently met with in returning from the summit of Elbrouz. This rock consists of a grayish-white feldspar cement, enveloping grains of hyaline quartz, small needles of amphibole, and scales of black mica. The cement crystallizes at some points, and then presents the structure of glassy feldspar. This rock exhibits the same stratiform structure which belongs to all the lavas of the Caucasus, and which gives to all these rocks the appearance of having flowed in a certain direction. On the banks of the upper Malka we still meet with large blocks of red and green jasper enveloped in the same black vitreous trachyte which we have above described.

In all our journies in the Caucasus, I have met with granite only in the hollows excavated by the rivers; but we know

from the observations of MM. Engelhardt and Parrot, that it is found in interlaced strata in the argillaceous schistus. There have been brought to me pieces of granite and gneiss detached from a rock near the source of the Malka. According to the information I have obtained respecting the position of this granite, it occupies all the bottom of the vallies, never rising to a great height, and is co-ordinate with the argillaceous schistus. I have been assured that the black trachyte of the central chain has been seen expanded over several of the beds of granite which are met with at the foot of Elbrouz, while ascending the river Malka.

The granite which I have met with in the bed of the Malka is composed of common white feldspar, with a little quartz and silvery mica. The mica is disseminated in it in laminæ pretty large and distinct, which gives to this granite the appearance of gneiss.—See our last Number, page 351.

ART. XV.—*Historical Notice respecting the Inflammation of Phosphorus in Vacuo.* In a Letter to the Editor by Professor MOLL.

MY DEAR DOCTOR,

Utrecht, 26th May 1831.

THERE is a statement in one of the last numbers of your *Journal* which, if not altogether erroneous, is certainly at any rate *not quite correct*. I hope you will excuse me if I attempt to set you right. It concerns the combustion of *phosphorus in vacuo*, or as you express it more justly, in No. 8, p. 370, of your *Journal*, in a *partial vacuum*. That experiment is attributed by many to a countryman of mine, of the name of *Van Bemmelen*, who by a misnomer in English and French periodicals has been sometimes called *Van Bemmeleer*. That worthy gentleman, since deceased, cannot claim by any means the honour of the invention of this very singular experiment. This honour belongs entirely to my venerable and celebrated friend Dr Van Marum, who as far back as 1794, observed this curious phenomenon. He published his experiments as early as 1797 in a Dutch collection of chemical tracts, called *Kasteleyn, Chemische Oefeningen*, vol. iii. p. 249. Soon after he gave an account of them in *French*, in the *Description de quelques*

Appareils Chimiques du Musée de Teyler, No. 10, p. 40. This was repeated in the *Annales de Chimie*, T. xxi p. 158, and in German in Gren's *neues Journal der Physik*, T. iii. p. 96. Mr Van Marum's experiment was this, that phosphorus, lightly wrapped in dry cotton, or in cotton powdered with resin, would inflame in vacuo of a good air-pump.

In 1801, the late Dr Van Bemmelen published an account of the repetition of Dr Van Marum's experiments. He found, however, that phosphorus powdered with resin, or sulphur, without being wrapped in cotton, would inflame equally well, and that the experiment succeeds also when phosphorus is covered with volatile oil of cloves. But neither Dr Van Marum, nor his friend Dr Van Bemmelen could succeed in inflaming phosphorus, *not* covered by or *wrapped* in some other substance; this appears to have been effected by the transatlantic philosophers, according to the very brief account of their proceedings given in the *Edinburgh Journal of Science*. Van Bemmelen's tract was in part printed in German in 1828, in the late Professor Gilbert's *Annalen der Physik*, neue folge, T. xxix p. 268. Nearly at the same time in 1827, another paper, on the same subject, was published in Dutch by a clergyman of the name of Koning, and this tract, to which a golden medal was awarded by the Zealand Society of Arts and Sciences, is to be found in the second part of the fourth volume, of their Transactions. (*Nieuwe Verhandelingen van Zeeuwisch Genootschap*. 4de deel, 2de stuk.) I have seen the experiment frequently repeated in public lectures, I often repeated it myself, but I never saw or heard of a satisfactory explanation of so uncommon a phenomenon. This difficulty of accounting for the result was perhaps the cause why the experiment was scarcely ever mentioned in any book on natural philosophy or chemistry; and except in Holland, it does not appear that the experiment was much noticed or even repeated. The thing appeared almost forgotten, when Mr Berzelius was struck by the abstract of Bemmelen's experiments in Gilbert's *Annals of Physics*; of this abstract another abstract was made in Mr Berzelius's very valuable yearly account of the state and progress of natural science; from thence it gradually penetrated into almost all the scientific journals of Europe. As, however, Mr Berzelius had made

unfortunately the mistake, of attributing the invention of the experiment to Mr Van Bemmelen, all the other editors of scientific journals followed in the same track, and Dr Van Marum has a fair chance of being deprived of the credit of an invention, which he made fully thirty-seven years ago, and of which accounts were printed in the most popular French and German scientific periodicals of the time. My venerable and much respected friend feels justly vexed at this, and I trust to your *love of truth and justice* the care of setting the public right on this subject.—Yours very sincerely,

G. MOLL.

ART. XVI.—*Remarks on Dr Goring's Observations on the use of Monochromatic Light with the Microscope.*

THE value of practical science, and the superiority of the results of accurate experiments to the deductions of theory, are universally acknowledged, and by none more readily than those who are principally occupied with abstract research. When the calculated results of general laws are found in direct opposition to those of accurate observation, the philosopher is on the eve of some important discovery; but whenever such an opposition presents itself, he will be careful to repeat his experiments, and he will not venture either to assail the correctness of a well established and universally admitted law, or to seek for the new principle on which the discrepancy depends, till these experiments are placed beyond a doubt. While we admit, therefore, the transcendent value of practical science, we must so far take the part of theory as to insist upon a rigorous scrutiny of the facts by which it is opposed.

As I was the first person who recommended the use of artificial monochromatic light for microscopical observations, by arguments drawn not only from theory but from immediate and frequently repeated observations, and as Dr Goring's observations, as recorded in a preceding article, are, if correct, entirely subversive of mine, it is necessary that I should relieve science from the opprobrium of these contending opinions. As Dr

Goring's experiments were made with monochromatic light such as his prism could give him, while mine were made with the infinitely superior illumination of the yellow light of a monochromatic lamp, I might have satisfied myself with the reply, that the results which he obtained with the one could not possibly invalidate those which I obtained with the other; but this would have been a virtual evasion of the difficulty, for I am convinced that monochromatic light properly obtained by the prism is as well fitted for microscopic vision as that of a monochromatic lamp, provided we use a portion of the prismatic spectrum that has the same limited range of refrangibility. This, however, is almost impossible, and hence the great superiority of artificial yellow light is undeniable.

The conclusion at which Dr Goring arrives is, that white light is as good for microscopical vision as the monochromatic light which he obtained from the prism,—that such light is not monochromatic,—and that a monochromatic light is still a desideratum. The proof which he gives of these conclusions is this: When any of the prismatic colours were employed to illuminate the microscopic object, the object was fringed with prismatic colours. Of the accuracy of this observation we cannot doubt; but it is manifest that the prismatic colours employed were *compound*, and were either *formed by an imperfect prism, or mixed with extraneous light*. Instead of suspecting the goodness of his prism, or his method of observation, Dr Goring is led to infer that *a certain quantity of white light enters into the constitution of each of the seven coloured rays, and may be rendered manifest by the dispersive powers of prisms and lenses*.—This result is in direct opposition to all Sir Isaac Newton's experiments with the prism; and if Dr Goring will employ the methods suggested by Sir Isaac for obtaining homogeneous light, he will discover the origin of his mistake. As this, however, would be attended with some trouble, we beg to suggest the following experiment which we have made fifty times with the same result. Take an excellent prism, capable of showing Fraunhofer's lines in the spectrum, and having admitted the sun's light through the narrowest practicable opening between the edges of the window shutters, fix the prism vertically at the distance of twelve or fifteen feet, so

as to see through it a prismatic spectrum of the luminous aperture. Take another prism equally good, and view through it any portion of the first spectrum, by refracting that portion towards the violet extremity, and it will be seen that it is perfectly unchanged in its nature and colours by the second refraction. The lines will be seen as distinctly as before, and not a vestige of any other colour will be seen but that of the coloured portion itself.

In reference to the perfection of monochromatic vision, the following experiment will be decisive. Place a printed page of a small type upon a table illuminated by the sun. Standing at the distance of some feet, view this page through a prism so that the plane of its refraction is parallel to the lines in which the types are arranged. The print will be entirely illegible, each letter being blurred with a prismatic spectrum of the white spaces between the sides of the types. Let the page be now illuminated by the light transmitted through a leaf of *red* paper, and the words and letters will be distinctly legible, though still indistinct on account of the red light not being homogeneous. Shut the window shutters, and illuminate the page with a monochromatic lamp: The words and letters will now be beautifully sharp and distinct, and excepting their slight expansion in one direction by refraction, they will be as clearly seen as if they were viewed through a plate of parallel glass, or with the eye alone.

If Dr Goring will therefore substitute the monochromatic lamp in place of the sun, we have no doubt that he will change his opinion on the subject of monochromatic vision, and if his health will permit him to devote his acute mind, and apply his great practical knowledge to microscopical discovery, we pledge ourselves that he will find monochromatic light the most powerful instrument of discovery which optical science can at present command.

In the use of single chromatic microscopes, a great deal may be done to improve the image, and even free it from colours when particular kinds of objects are used. Let it be required, for example, to determine the place of an object consisting of close parallel lines, in order to obtain the most perfect vision of it by a single chromatic lens.—The lens should, in the *first*

place, have its axis horizontal; because in this case the eye is in its natural position, and the fluid which lubricates the cornea is more evenly diffused over it by its gravity. If the axis of the eye were vertical, the fluid would tend to accumulate at the centre of the cornea, and produce a less perfect refraction of the incident rays. The direction of the lines in the microscopic object must be vertical, or parallel to the lines in which the fluid descends over the cornea, because in this position their sharpness will be least affected by the descent of the fluid. And, *lastly*, the direction of the lines should be in that of a diameter of the field of view passing through the axis of the lens, because in this position *all colour whatever will disappear between the lines*, as the plane of refraction in the lens is coincident with the lines; a case in which the largest refracting angle of the most dispersive prism is incapable of producing any colour in a line of light. When the lined object is placed out of the centre of the field, the prismatic colours will immediately appear at the edges of the lines; but this species of colour may be corrected by the opposite refraction of a prism; or even by looking past the edges of the finger, so as to produce a compensation by the chromatic effect of the eye. This last resource will of course obstruct a good deal of light, and is mentioned chiefly for the sake of illustration.

We leave it to the mathematicians to deal with the remaining part of Dr Goring's paper, which displays great practical knowledge of optical instruments, and contains much original and valuable information;—but we cannot avoid touching upon one topic. Dr Goring has in page 67 quoted a passage from Mr Coddington's preface to his *Treatise on the Eye and Optical Instruments*, which has no doubt compelled him to take up the gauntlet thus thrown down to every improver of the microscope. The following is the passage referred to, which has given just grounds of offence, not only to those who have successfully devoted their time and their fortune to the improvement of the microscope, but to those also who feel that English science might justly boast of its achievements in this department of optics. While the French and Bavarians have been carrying off from us the palm in achromatic telescopes, it was some con-

solution that we still surpassed them in microscopical improvements.

“On the subject of telescopes,” says Mr Coddington, “I have done little more than detail and explain to the student the improvements which the talents and industry of Huygens, Dollond, and Ramsden, had long since made available in practice. With regard to the microscope, I may perhaps lay claim to a little more. That instrument had, till very lately, been almost entirely neglected since the days of Campani, *and I am inclined to doubt whether the intellectual and manual labour* which has within a few years been bestowed upon it have been on the whole profitably directed, since they have left wholly untouched a source of error perhaps the most important of all, namely, the obliquity of the refraction at the object-glass.”

We entirely agree with Mr Coddington in the opinion, that the microscope has been almost entirely neglected since the days of Campani; but we differ widely from him in his estimate of the value of the *intellectual* and *manual* labour which has been lately bestowed upon it.—We regard the last twenty years as the golden age of microscopical improvement; and in proof of this position, we shall enumerate some of the *intellectual* and *manual* labours by which this period has been so remarkably distinguished.

1. Dr Wollaston's introduction of central pencils by means of two hemispheres separated by a diaphragm.

2. Dr Brewster's grooved or excavated sphere, very inappropriately called the bird's-eye lens by Mr Coddington.

3. The catoptric hemisphere, by which the magnifying power of a hemisphere is doubled.

4. Mr Herschel's double lenses free from spherical aberration.

5. Dr Wollaston's microscopic doublet.

6. Dr Wollaston's improved method of illumination.

7. Introduction of monochromatic light.

8. Introduction of microscopes made of garnet, sapphire, and diamond.

9. The improved Amician reflecting microscope.

10. Selligie's idea of combining achromatic object-glasses.

11. Dr Goring's various improvements on the microscope, as described in this *Journal* and elsewhere; and his introduction of test objects. See this *Journal*, No. xx. Old Series, p. 327, 360; No. ii. New Series, p. 64, 353; and No. viii. p. 244. See also the *Treatise on the Microscope* published by Dr Goring and Mr Pritchard.

12. Mr Lister's discovery of the double aplanatic foci, and variable aberration of achromatic object-glasses having their inner curves in contact.

In the preceding enumeration we do not pretend to give an exact list of recent microscopical improvements: We have merely mentioned those that happen to be best known to ourselves.

With regard to *manual* labour, we may give the following enumeration:—

1. The new and difficult art discovered by Mr Pritchard of making diamond lenses.

2. The great skill exhibited by Chevalier, Tulley, Amici, Utzschneider, and others, in constructing minute achromatic object-glasses.

3. The art of giving the elliptical form to small specula 3-10ths of an inch focus, and of the same aperture, as perfected by Mr Cuthbert. See this Number, p. 74, note.

This enumeration of facts will, we hope, relieve the philosophers and opticians of England and other countries, from the charge of having given an unprofitable direction to their intellectual and manual labours in the improvement of the microscope. That their inquiries have been pursued in the right direction, may be safely inferred from the extraordinary success with which they have been attended.

ART. XVII.—*Notice of the Discovery of the Termination of the Niger, in the Bight of Biafra.*

THE discovery of the termination of the Niger has long been a problem of some interest in geography. The direction of the river seems to have been traced to a point in 10° of north latitude, and 5° of west longitude, a little to the south of Yaory or Youri, the place where Mungo Park appears to have been

wrecked. At this point the Niger, or the Kowara or Quarra, as it is there called, is within less than 4° of the Bight of Benin, in the Gulf of Guinea, but no correct information could be obtained respecting its future course. According to information given to Major Denham it then took a direct *easterly* course towards the Nile, and the very same direction is assigned to it in Sultan Bello's Map of Central Africa. An able writer in the *Quarterly Review* supported this view of the subject with great ingenuity. According to the hypotheses of Major Laing, and from information communicated by the Sheikh of Gadamis, the Niger went *westward*, passing through the mountains of the Moon, and terminating in the River Volta, in the Bight of Benin, in north lat. $5\frac{1}{2}^{\circ}$ and east long. 1° . Captain Clapperton was led to give it a course straight south from Youri, and terminating in the Bight of Benin, to the east of Lagos, in north lat. $6\frac{1}{2}^{\circ}$ and east long. $4\frac{1}{2}^{\circ}$; while according to the hypothesis of Reichard, a German geographer, it went a little farther to the east, terminating in the Bight of Benin, and forming the great River of Formosa in Benin, in north lat. 4° , and east long. $5\frac{1}{2}^{\circ}$. Other travellers, such as Mungo Park and Captain Maxwell, identified it with the Congo or Zaire.

Of all these hypotheses that of Reichard is the nearest to the truth, as it has been found to terminate in the Nun or Brasse River, in the Bight of Biafra, in north lat. $4\frac{1}{2}^{\circ}$ and east long. 9° , just at the great angular point of the African continent, a little to the north of the Island of Fernando Po.

This great discovery was made by the two Landers, one of whom accompanied Captain Clapperton, and attended that adventurous traveller on his deathbed at Sockatoo. In 1830, Mr Landers, accompanied by his brother, sailed to Africa, and succeeded in tracing the Niger to the sea. The following account of the discovery was communicated to the Editor of the *Literary Gazette* by Mr Fisher.

*“ His Majesty's Ship Atholl, at sea,
Bight of Biafra, Feb. 2, 1831.*

“ DEAR SIR,—I take the opportunity of writing you a few lines by a vessel that we have just now met on her way to England. My object in writing in this hasty manner is to ac-

quaint you that the grand geographical problem respecting the termination of the Niger is at length solved.

“ The Landers, after having reached Youri, embarked in a canoe on the Niger, or, as it is called there, the Quarra, and came down the stream until they reached the sea, in the Bight of Biafra. The branch by which they came to the coast is called the Nun, or Brasse River, being the first river to the eastward of Cape Formosa. On their way down the river, they were attacked by the Hibboos (a fierce nation that inhabit its banks), and made prisoners, or rather captives ; but the King of Brasse happening to be in that country buying slaves, got them released, by giving the price of six slaves for each of them. In the scuffle that ensued at the time they were taken, one of them lost his journal.

“ Whilst at Youri they got the prayer book that belonged to Mr Anderson, the brother-in law and fellow-traveller of the celebrated Mungo Park. They were upwards of a month at Fernando Po, whence they embarked, about ten days ago, in an English merchant vessel bound to Rio Janeiro, on their way to England. From their taking that circuitous route, I am in hopes that this will reach you before they arrive, by which you will probably have it in your power to give the first news of this important discovery.

“ I do not recollect of any thing else to acquaint you with that is worthy of notice ; and even if I did, I have no time to mention it, as the boat by which I send this (to the vessel) is just this moment ordered away.

“ I must therefore bid you adieu for the present ; and believe me, dear Sir, yours very sincerely,

“ ALEXANDER FISHER.”

ART. XVIII.—*On the Diamond Mines of Panna in Bundelkhand.* By CAPTAIN JAMES FRANKLIN, First Bengal Cavalry, M. A. S.

THE geological position of the matrix of the diamond, being still a question in the history of that gem, the following notice on the diamond mines of Panna, will not, I trust, be unacceptable.

Report says, that they were first discovered in the time of Raja Chitrasál, who ruled at Panna, in the reign of the emperor Aurangzeb, but that period being a troublesome æra in the annals of the Bundelas, it is supposed, that they were not efficiently opened, until the time of his grandson Subha Sinha.

Their situation is peculiar, being confined to a small portion of the great belt of sandstone which extends from Rotásgerh, through the provinces of Boghélkhand and Bundélkhand, until it is finally covered by the overlying trap of Malwa and Sagar; this, however, is but a small part of the extent of this formation, for the break at Rotásgerh is merely an hiatus occasioned by the original current of the Soan valley, which doubtless swept away every vestige of this rock, until its force was turned aside by the projecting points of the Vindhya range, near Monghir,—after which in the Rájmahal hills, the sandstone again appears as before,—and from that point it may be traced throughout the whole of the peninsula; it is the depositary of the diamond at Panna—and I have no doubt that the rock mines both of Sembhelpur and Banganpilli, though far asunder, will ere long be found to belong to the same formation; in the meantime, the following facts which have fallen under my own observation, on my route from Belári to Ajayagerh, may serve to identify the class and character of the rock which contains the matrix of the diamond of Panna.

The first part of the route (or from Belári to Ajayagerh) crosses the most lofty portion of the sandstone belt, usually called the Bandair hills—which, without exception, is entirely composed of argillaceous sandstone, either mottled or streaked—and opposite to the village of Piperiya, below the Ghat of that name, I observed the sandstone reposing on beds of slaty marl.

Having descended the Bandair hills by the Ghat of Piperiya, I came upon the second range, which, like the former, is composed of sandstone, but the surface of its plateau being covered with a stratum of lias limestone, the sandstone can only be traced in the beds of rivers, or in small protruding elevations, until it emerges from beneath the limestone and forms the counterscarp of the Panna hills, where it is variegated and

friable, and the marly slates are again visible in the hills which overlook the town of Panna.

The lowest portion of the range, or that which is called Bindáchal, is the peculiar habitat of the diamond, for it is not found in any other part, except on the platform of this range, or on the counterscarp of the second, and it is proved by the waterfalls, that this range also is entirely composed of sandstone. For instance, the cascade of the Ranj river shows a series of sandstone interstratified with slate clay three hundred and ninety feet thick. All the other waterfalls present similar appearances, and that of the Bágin river, penetrating deeper than the rest, exhibits a fine section; here the sandstone is distinctly interstratified by a succession of layers of slate clay, the uppermost of which having a marly base is thickest, and the descending strata becoming more indurated, containing more mica, and gradually diminishing in thickness, dwindle away finally into mere partings, and in their progress to this attenuated state, they assume characters so various, that in some instances, it is difficult to distinguish them from the older schists. The sandstone also changes, gradually becoming silicious, and at the bottom it closely resembles some varieties of quartz rock, but the horizontal position of the beds is constantly preserved, and in all the glens, particularly in that of the Bágin river, black bituminous shale crops out from beneath the sandstone. I excavated this shale to the depth of six feet, but having no other means than such as I could procure on the spot, the influx of water soon overpowered my operations. I found, however, that the bituminous quality of the shale increased,—fragments of it, throwing out strong shoots of flame when ignited, and I was disposed to think that coal was not far distant.

I have ventured to call this formation *new red sandstone*, considering it in the same light as the series of rocks so termed in England, and it would appear, that this denomination is in some measure corroborated by other facts, in other portions of the same range of hills, but principally by the proof of its saliferous nature. It has been shown, that at the village of Kattrá, the soil is impregnated with salt, which is there, and in many other adjacent villages, extracted by the native

process of lixiviation, such is the case also on the banks of the Tons river, and Mr Stirling, who published an account of the diamond mines of Panna, remarks, that salt abounds in the soil at the foot of this range, opposite Allahabad, and between that place and Mirzapur. These facts, therefore, together with the general horizontal position of the beds, the existence of lias limestone reposing upon them, the distinct interstratification of a series of slate clay, and above all, the cropping out of bituminous shale from beneath the whole mass, would appear to justify the use of the term which I have applied.

I have been thus prolix, because it is of importance that I should be clearly understood with regard to my nomenclature, and, if I am wrong, my own description may, perhaps, serve to correct my error. Having thus premised, I shall now proceed to give as brief a description as I can of the mines in question.

The natives describe the mines by using the terms *chíla*, or superficial, and *gahira*, or deep, and the matrix they call *madda*; the rocky matrix of the deep mines is always a conglomerate, and, if it is a gritstone with a silicious cement, and its pebbles are of ancient rocks, and waterworn, it is termed *pakka* or mature; but if the cement is argillaceous, and its pebbles are of more recent rocks, it is then called *kacha*, or immature; the matrix of the superficial mines is universally called *Lálkalkru*, or red ironstone gravel, mixed with ferruginous sand or clay. This gravel is waterworn, and sometimes quite rounded like swan shot, and when found in the fissures and interstices of the upper sandstone, it is mixed with ferruginous sand, but, on the other hand, when imbedded in ferruginous clay, it is usually found covered with vegetable soil, and reposing upon slaty marl;—sometimes, however, it is surmounted by a stratum consisting of particles of common *kankar* imbedded in yellow clay, which occasionally mingling with it, forms another description of matrix, which, being calcareous, is called *hadda*; the diamonds of the glen of the Bágin river, have evidently been transported thither from their native beds, and in all probability the gangue in which they now rest in the basin of the waterfall, greatly resembles the cascalho of the Brazils, or that of Sambhampur, in Southern India.

The *pakka*, or rocky matrix, is very limited, stretching generally from Kamariya to Brijpur, along the course of the Bagin river. It is excavated at Kamariya, Brijpur, Bargari, Myra and Etwa; there is also a small deposit of it near the town of Panna, but at Brijpur, from the effects of denuding causes, it lies at the surface, and a very satisfactory section of it is laid bare in the bed of a small rivulet about one mile west of the village, where it appears to be a gritstone, composed of white quartz gravel, cemented by silicious matter, and containing rounded pebbles of quartz, jasper, hornstone, Lydianstone, &c. Thus it forms a conglomerate, which passes by gradual transition into silicious sandstone. It is readily distinguished from its associated rock, differing greatly from it, in as much as the sandstone in which it is found has a martial argillaceous cement, and closely resembles that which forms the upper layer of the cascade of the Bâgin river.

Kamariya Mines.—The most noted mines of this description of matrix are those of Kamariya and Panna; at the former place they are on an average about fifteen feet deep, and in one which I examined, the beds of slaty marl were two feet below the surface, a thin stratum of red ironstone gravel imbedded in ferruginous clay, and vegetable soil, were their only covering; they differed in no respect from those of Piperiya Ghat, they were marly, slaty, slightly micaceous, interstratified with thin laminæ of sandstone, and associated with calcareous slates, which were dendritic between their partings, and although their general colour was bluish-green, or greenish-grey, yet there was a sufficient mixture of red to characterize them; they were about twelve feet thick, and immediately below them was the rocky matrix of the diamond.

The conglomerate is here as at Brijpur, a gritstone containing pebbles of quartz, both white and green jasper, hornstone, Lydianstone, &c. and it is worthy of remark, that when the green quartz pebbles abound, it is considered a good sign, and so also when the gritstone is slightly ferruginous, the matrix in these mines reposes on compact sandstone.

Panna Mines.—The mines of Panna are of the same kind: here also the stratum beneath the vegetable soil is red ironstone gravel, below which are beds of slaty marl, better characterized

if possible than those of Kamariya, then follows the diamond matrix, which differs in no other respect from that of Kamariya or Brijpur, except that it appears to contain a little more ferruginous matter; its pebbles are the same, its cement the same, it has the same peculiarity of containing green quartz nodules so highly esteemed as an augury by the natives, and its floor is of the same description of sandstone.

These mines vary in depth from twenty to fifty feet, and, owing to the stratum of the matrix being thinner, (sometimes scarcely a span thick,) they cannot be worked laterally as at Kamariya, they are therefore more expensive, but their produce is said to cover the outlay and yield a profit. They are consequently esteemed, and hold a reputation nearly equal to those of Kamariya.

Sakeriya Mines.—The *kacha*, or immature matrix, is excavated at the villages of Sakeriya and Udesna, both situated on the counterscarp of the Panna hills. It contains rounded pebbles of quartz, jasper, Lydianstone, &c. but with these are mixed more recent pebbles of white sandstone. It contains also much white quartz gravel, called by the natives *detla*, but the cement of the conglomerate, instead of being silicious, is a yellowish white clay, soft and plastic when in its natural bed, but capable of acquiring the consistency of mortar when exposed to the atmosphere, and when it contains ferruginous matter it is considered a good sign. The quartz pebbles are of the fat and greasy variety, and the green kind so much esteemed in the rocky matrix, is here entirely wanting.

A shaft near Sakeriya which I examined, pierced through the following beds 1st, eight feet vegetable soil; 2d, eight feet *pīri matti*, or common *kankar*, imbedded in yellow clay; 3d, four feet *lalkakru*, or red ironstone gravel in ferruginous clay; 4th, two feet *detla*, or white quartz gravel; next followed sandstone, and then the *kacha* matrix. The thickness of the *detla* stratum is here considered a matter of augury; if it is too thick it augurs ill, as it is then supposed that the stratum of *madda* will be correspondently thin, or wanting altogether. It ought not to exceed two feet.

Udesna Mines.—Near the village of Udesna, the same kind of matrix underlies laterite, there called *macha*. The great

abundance of ironstone gravel and ferruginous matter strewed over this part of the country necessarily produced in former times, and no doubt still continues to produce, a great quantity of oxide of iron, which being washed away, and held in solution by the minor streams, has been gradually deposited in the channel of the Ranj river until it is now about ten feet thick, and immediately below it are the beds of *detla* and sandstone, and the matrix as above-mentioned. This matrix does not require to be broken, the clay is easily separated by washing, and the expence of working the mines is consequently lessened, but still they are not considered so certain in their return as those of the rocky matrix.

Superficial Mines.—The *chíla*, or superficial mines, are to be found in every part of the diamond tract, excepting only a circuit of about five miles from the cascade of the Bágín river, where it appears that denuding causes have swept them away and all their contents into the glen of that river. Their matrix is always red ironstone gravel in ferruginous sand or ferruginous clay. Their geological position, with regard to the descending series, appears to be remarkably well defined, for they are actually to be found on the verge of two cascades, having 400 feet of sandstone beneath them; when this matrix fills the fissures and interstices of the upper sandstone, angular fragments of the rock are mixed with it, the corroding influence of the oxide of iron appearing to have detached them as well as to have disintegrated and oxidated a portion of the rock, so that the gravel and fragments are imbedded in sand so highly ferruginous that it resembles the rust of iron; but when, on the other hand, it is imbedded in ferruginous clay, it contains no fragments of sandstone, and is constantly found overlying slaty marl or sandstone, or *detla*, as in the instances above-mentioned; with regard to the ascending series, its geological position seems also to be well defined, for, if the two strata of red ironstone gravel and *kankar* occur together, as they do at Sakeriya, it always underlies the calcareous bed, and their line of separation is distinct, so that, when they happen to mingle as at Bangla, the matrix acquires a new name, and is then called *hadda*.

These mines rarely exceed five or six feet in depth, and are

often much less; with regard to their produce I am inclined to think that they are very precarious, notwithstanding some of the largest diamonds have been found in them; it is common to hear complaints of having found nothing for many months, and to me they appeared like a lottery in which there are a few prizes and many blanks—they have an advantage in requiring little or no outlay, and are consequently wrought by all classes, but it is not unlikely that more capital has been sunk in them in the shape of labour than has ever been returned.

The diamond is occasionally, though very rarely, found on the surface, nor is it improbable that some lucky chance of this kind may have led to the discovery of the mines.

Mines of Transported Diamonds.—The above is a brief account of all those matrices of the diamond in the Panna district, which fall under the denominations of *madda*, *lálkakra*, or *hadda*; but there are others where the gem is found in deposits with which it appears to have been swept away from its native beds, as at Majgoha and in the glen of the Bágin river; the mines of the former place are peculiar, and require separate mention, but in those of the glen, the diamond is found under rocky debris, both on the banks and in the bed of the river, and also in the basin which receives the cascade; its matrix in this state, is a confused mixture of red ironstone pebbles, angular fragments of sandstone, and pieces of common *kankar*, heaped together in ferruginous sand or clay, the detritus in fact of its original gangue; and the mines of course have a great resemblance to the superficial mines above-mentioned, but they are said to be rather more productive, and there is great reason to believe that the basin of the cascade has never yet been emptied or excavated except to a trifling extent.

Majgoha Mines.—The mines of Majgoha are in the western part of the diamond tract, and they may properly be called its western boundary; they are situated in a hollow resembling an inverted cone, which appears to have been excavated by the same process, (more powerfully applied,) which scooped out those resemblances to it in miniature, which are observable in the rocky beds of rivers; the diameter of the vortex is

about 100 yards, and its depth (I presume) cannot be less than 100 feet ; on its periphery, superficial mines are wrought in sandstone, but the cavity of the chasm is filled with green mud, containing calcareous matter, such as I can find no apt similitude to, except by supposing it to be the abraded matter of the same marly slates as those which occur in the mines of Panna and Kamariya, here deposited en masse, and there in slates ; this of course is mere conjecture, but if the vortex has been formed as I suppose it to have been, the matter could not in that case have acquired a schistose form ; be the facts of the case, however, what they may, this singular deposit fills two-thirds of the chasm, and at the top it has a thick crust of calcareous spar, which is indistinctly stratified, and contains portions of the green mud between its laminæ.

The diamond is rarely found in the calcareous crust, its habitat being in the green mud, and it is believed by the natives, that the deeper a shaft descends, the richer is the produce ; but although they are aware of this circumstance, their ordinary means have never enabled them to descend lower than fifty feet, the water at that depth overflowing their works, and compelling them to desist : this deposit, therefore, and that of the basin of the Bâgin river, appear to be two instances in which superior means might be employed, with effect, and perhaps with profit.

Mode of washing and searching the Matrix.—The mode of washing and searching is the same in all the mines, the rocky matrix alone requiring to be broken ; it is first thrown into a trench with water and shoveled and trod like mortar, and as the object is to wash away the clay, fresh water is thrown on and poured off repeatedly until the fragments are sufficiently cleansed, and, as a final purification, they are sifted on fine baskets, which completes the operation of washing ; they are then spread in a thin layer on a smooth floor plastered with clay or cow-dung, and when dry the whole is passed under the hand, and searched three several times, after which the fragments are thrown aside.

Reproduction of the Diamond.—The circumstance of diamonds being frequently found amongst these fragments after they have been thrown aside, has, perhaps, given rise to the

idea of their reproduction, and I was anxious to obtain the opinion of experienced natives on this subject: they admit it only in one instance, viz. at Majgoha, and even there, it is always ascribed to the spiritual agency of the founder of the *Mchdivi* sect, to whom those mines belong, but their more rational opinion is as follows, which I will give as nearly as possible in the words of my communicant:—"The object of washing is to free the rocky fragments from clay, and particularly to cleanse the diamond, so that it may readily be distinguished in the operation of searching, but with all our care we cannot always succeed; small diamonds frequently retain their covering, and thus elude our search in the first scrutiny, nor can they be discovered afterwards, until the coating which concealed them is worn away; hence it happens that diamonds are found amongst fragments which have been searched and thrown aside, but it is observable that small diamonds alone are so found, and that they rarely exceed the weight of half a troy grain."

With regard to Majgoha I am inclined to think that the above opinion applies with great force. The matrix of these mines contains calcareous matter, and it is no easy attainment to wash away a calcareous incrustation by using water alone; whenever, therefore, such an occurrence takes place, the diamond might not only elude a first search, but a series of searches, and even for a series of years, until the coating which enveloped it was worn away.

Description of the Diamonds.—The diamonds of the Panna mines may be classed according to the following arrangement, using native denominations: 1st, *Lilwaja*, transparent, colourless, having no tinge except the azure which is reflected in a drop of distilled water; it is so scarce that only one specimen was to be found in the town of Panna.

2d, *Banspati*, *Motichar*, *Ghiya*, or *Maska*: these kinds are common, the first has a greenish tinge, the second is also greenish, but varies to a pearly cast; the third is yellowish and of a greasy or resinous lustre, as its name implies;—the crystalline form of this class is very distinct, exhibiting frequently the regular octahedron as perfect as if it had been shaped by an artist, the dodecahedron is also common, and

so is the spheroidal, arising apparently from the convexity of its faces, and the obtuseness of its edges, the average price of this class is thirty Srinagari rupees, for diamonds of one *retti* weight, 35 for two, 40 for three, 45 for four, and 50 for those of five *retti* weight.

3d, *Sambarra* and *Charchara*: these are they which have given rise to the belief that the Panna mines produced only table diamonds; the specimens I saw were quite irregular in their crystalline form, appearing as if they had been broken by a violent blow; but they invariably cleave into thin tabular laminae, and, as they are generally of a good water, and sell for a low price in comparison with the others, the Panna jewellers appear to find it more profitable to work them up, by setting them in rings or other ornaments; their one *retti* price is twenty rupees, increasing according to weight as above stated.

4th, *Bengala pashmi*, *Pira* and *Matta*: these are yellowish green, yellow, and clove brown, and their crystalline form is multiform, the price of the one *retti* gem is fifteen rupees, increasing as above.

5th, *Rekatberar*: this is the rose-coloured variety, its crystalline form is also multiform, it is not esteemed, and its single *retti* price is twelve rupees.

6th, *Kala*, *Garas*, or *Jalidar*: the first is black or very dark brown, and the second, as its name implies, includes all diamonds that are flawed or appear to contain filaments like a spider's web: these varieties are here termed *Kaffiya*, or scum, but in England they are called *bort*, and there they are used in the arts for diamond dust to an extent unknown in this country, their price varies according to the size of the stones; but as they seldom, if ever, exceed one *retti* weight, the worst kind may be purchased for eight, and the best for ten rupees the *retti*.

The above list contains the principal names classed according to their relative value; but there are others, apparently founded on fancy alone, a recital of which would embarrass rather than throw light on the subject; the prices also must be considered variable, a purchaser coming suddenly into the market would as infallibly occasion a rise, as a deficiency of demand would create a depression; a purchaser therefore should

fix himself on the spot, and make his purchases gradually, by so doing, he would at least save the profits which now go to the merchants of Benares.

Revenue of the Mines.—The revenue of the mines is divided among the Rajas of Panna, Banda, Chircari, and Jatipur, but by far the largest share belongs to the former. According to my calculation the Panna division amounts to about 26,000 rupees per annum, but according to Raja Pertar Sinh, who is the efficient manager of the Panna state, it is 30,000 rupees, and as his authority is likely to be nearer the truth than mine, I do not hesitate to adopt it; this revenue is derived from a tax, originally fixed at one-fourth of the value of all diamonds found in these mines below a certain weight, which, I believe, was rated at eight *rettis*, but the tax now levied is said to exceed this rate, and on diamonds above the eight *retti* weight there is no stipulation; taking therefore the aggregate of the Banda, Chercari and Jaitpur shares, as equal to a fourth of the revenue derived by the Raja of Panna, it will not be too much to suppose that the produce of the mines amounts to about 120,000 rupees per annum.

I have now detailed with the utmost fidelity all the circumstances relating to these mines as they occurred to me at the time I examined them, and have endeavoured to throw into my narrative as much perspicuity as the subject is capable of; still, however, the diamond is too important a mineral to be passed over in a hasty manner, and I trust I shall be excused if I here indulge in a few general observations.

General Remarks.—1st, It was formerly supposed that diamonds were always found at the same level above the sea, and it still remains to be proved whether or not there is any truth in the hypothesis, the following barometrical heights are deduced from actual observations made by myself.

Kacha Matrix.

Source of the Ranj river near Udesna,	-	1496 feet.
Floor of the mines of Sakeriya and Udesna,		1470 „

Pakka Matrix.

Floor of the mines near Panna,	-	1300 feet.
Bed of the Ranj river due east of the above,		1300 „
Top of its cascade near Ranipur,	-	1240 „

Source of the Bagin river near Urki,	-	1420 feet.
Floor of the mine of Kamariya	- -	1380 „
Bed of the Bagin river due south of them,	-	1380 „
Floor of the mines of Brijpur,	- -	1260 „
Bed of the Bagin river exactly opposite,	-	1250 „

Matrix swept away.

Top of its 1st cascade,	- - -	1120 feet.
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Transported Diamonds.

Bottom of 1st cascade,	- - -	900 feet.
Bottom of 2d cascade,	- - -	700 „

From this list it would appear that the rock matrix of the diamond in the Panna mines has been swept away at an elevation of 1100 feet, and that its lowest position in situ is between 12 and 1300 feet above the sea.

2d, The contracted limits of this diamond tract has already been mentioned as a peculiarly striking circumstance; the same kind of sandstone as that in which the diamond is found, extends far beyond those limits, and why does it not contain diamonds also? to this question I can only reply by the following explanation: *on the north*, the scarp of the Bindáchal hills rests as a point d'appui on a low ridge of sienitic granite, and the plains of Bundelkhand exhibit primitive rocks throughout—therefore, excepting transported diamonds, none can be expected in that quarter; *on the south*, lias limestone stretches along the outline of the counterescarp of the second range of hills—and here again (excepting the diamonds of the counterescarp) none are ever found—being perhaps buried by the overlying limestone; *on the west*, the sandstone becomes thinner, being often little more than a mere capping; the conglomerate form is also frequent, but in the diamond tract it is remarkable that there is no other conglomerate than that which contains the diamond; moreover, black bituminous shale rises to the surface near the village of Sahigerh, though in the diamond tract I have never seen it with less than 400 feet of sandstone, resting upon it; *on the east*, the sandstone continues pretty much the same, and I cannot offer any satisfactory reason why diamonds should not be found east of the Cheyla Nadi, which at present is considered to be their eastern boundary.

3d, I have endeavoured to show that the rocky matrix of

the diamond of Panna is situated in sandstone, which I imagine to be the same as the *new red sandstone* of England; also, that, (if the transported diamonds are excepted,) there is at least 400 feet of that rock below the lowest diamond beds; and, further, that there are strong indications of coal, underlying the whole mass; how far this may agree with the geological position of the same description of mines in Southern India, will best be seen from the following extracts.

As far as I understand Dr Heyne in his tracts on India, pages 103-4, the hills which surround the rock mines of Banganpilli, are composed of slate clay, and his account of them reminds me much of Panna, he says, "they are straight at top, and usually level for some extent," so that even villages are built on them—he says also, that "the water of the wells is brackish," a strong indication of their saliferous nature, and further, "that the country about Banganpilli is sandy and stony, and that the stones are chiefly conglomerates, composed of silicious materials.

With respect to the rock in which the matrix of the diamond is found, his description is as follows;—"the solid rock of the hills (which, by-the-bye, is not quite destitute of diamonds,) is an aggregate, consisting chiefly of a coarse grey hornstone, with rounded pebbles of the same species, but of a fine variety of stone, or of jasper, of different colours: at some depth, this rock becomes ferruginous sandstone, the grains of which are finely cemented together, and this kind of stone usually forms the roof of the floor of the mines; the floor is generally of a reddish brown colour, with shining particles, and strikes fire with steel;" again he says, through the solid rock the miners must make their way before they arrive at the diamond matrix.

Dr Voysey's account of these mines is, "that the diamond matrix," in its rocky state, is "a sandstone breccia;" it lies under "compact sandstone, differing in no respect from that which is found in the main range, it is composed of a beautiful mixture of red, and yellow jasper, quartz, chalcedony and hornstone, of various colours, cemented together by a quartz paste, it passes into puddingstone composed of rounded pebbles of quartz, hornstone, &c. cemented by an argillo-calcare-

ous earth, of a loose friable texture, in which the diamonds are most frequently found.”

The apparent discrepancy in these accounts is not irreconcilable—but Dr Voysey is most distinct in his description; he says that the rock under which the diamond matrix is found, is compact sandstone, and that it differs in no respect from the sandstone of the main range. He did not see the floor, but Dr Heyne appears to have done so; and, if I understand him right, the floor is sandstone also, for he says, (page 105,) that the diamond bed is of the same nature with the rocks both above and below it, but is distinguished from them by its superior hardness, and that the floor is so hard that it strikes fire with steel, a peculiarity which equally applies to the Panna mines.—Dr Voysey arrived at the following conclusions.

1st, That the matrix of the diamond in the mines of Southern India is the sandstone breccia of the “clay slate formation.”

2d, That those found in alluvial soil are produced from the debris of the above rock, and have been brought thither by some torrents or deluge, which alone could have transported such large masses and pebbles from the parent rock, and that no modern or traditional inundation has reached to such an extent.

3d, That the diamonds found at present in the bed of the rivers are washed down by the annual rains.

I cordially agree with Dr Voysey in the general result of his conclusions, because I am satisfied that the same circumstances are applicable to the mines of Panna, but I nevertheless differ from him in two points; 1st, I could not trace any likelihood of diamonds being washed away by any natural causes now in operation, such as the annual rains—they are, in general, too deeply covered with soil, even in their most superficial beds, to admit of this conclusion, and such only as might have accidentally laid on the surface could be so transported. 2d, I cannot agree with his nomenclature with regard to “clay slate formation,” because he himself says, that in using the term clay slate, he does not mean the *Thonschieffer* of Werner, which is the only recognizable term for that rock, according to the Wernerian system, but excepting these two

points, I have found great accordance with his result, and am happy in having it in my power to express it.

4th, There is another circumstance to which I must advert, but I do so with diffidence, and under a hope that it will be considered merely conjectural. Dr Brewster supposes the diamond to have originated like amber, perhaps from the consolidation of vegetable matter, and that it gradually acquired its crystalline form, by the influence of time and the slow action of corpuscular forces: the late Dr Voysey adverted to this opinion in his account of the diamond mines of Southern India; and on the occasion of publishing an abstract of that paper in his *Journal of Science*, Dr Brewster observed that he saw no reason to alter his opinion: now, as the rock matrix of the diamond of Panna appears in some respects, though not altogether, to resemble that of Banganpilli, in Southern India, there would seem to be little chance of my conjecture being useful, still, however, as every opinion regarding the origin of this fine mineral is as yet theoretical, I will not withhold what occurred to me on this subject, though I again repeat that I offer it with great diffidence.

The theory of Sir James Hall on the consolidation of strata frequently recurred to me when examining the sandstone in which the diamond is found; I thought that I could discern much in favour of it, and particularly in the gradual changes of its nature, from the lower to the upper strata; now, if the principle of this theory is admitted to be correct and applicable universally, it follows of course that it must be applied here; and then it may be questioned how the diamond was preserved, under that degree of heat which must have been necessary to form its matrix the gritstone? In answer to this objection, I suggest, that the circumstance of calc spar, occurring in trap rocks is somewhat analogous, and if it is admitted that compression under the weight of strata, and a superincumbent ocean, had the effect of resisting the expansion of its carbonic acid, and constraining it to continue in combination with lime, might not the same principle be reasonably enough applied, to account for the preservation and detention of the elements of the diamond in the gritstone? and again, should it be further shown that crystals, such as those

with which we are familiar in nature, may be produced by slow cooling or other processes according to the above theory, may we not look to it also, to account for the crystallization of the gem?

This conjecture rests upon the truth or fallacy of Sir James Hall's theory, or on a modification of it, and when this theory is considered as the result of long and patient experiment, and the high reputation of its author is taken into account, it will require something more than limited observation, or ordinary ability, to answer its objections; my part, however, is merely the suggestion of a traveller, and I therefore conclude my paper by expressing a hope, that this important mineral may meet with more able investigation.

ART. XIX.—*On the Discovery of Vanadium in Scotland, and on the Vanadiate of Lead, a new Mineral species.* By JAMES F. W. JOHNSTON, A. M. &c. &c. Communicated by the Author.

IT is a remarkable circumstance, and illustrative at once of the wide diffusion of chemical knowledge, and of the progress of scientific chemistry, that the new metal *Vanadium* has been discovered in three different countries nearly at the same time, and without any communication between the several individuals by whom it has been observed and detected. First in order of time, Professor del Rio, of the School of Mines of Mexico, detected a new metallic substance in the brown lead-ore of Zimapan, to which, probably from its forming red salts, he gave the name of *Erythronium*. His results were not published, however, M. Collet Descotils, to whom specimens were transmitted, having pronounced it to be an impure chromium. Meantime Professor Sefström, of the School of Mines at Fahlun in Sweden, detected in an ore of iron a simple metallic body, which he named *Vanadium*, and of which he announced some of the properties about the end of the past year. The metal of del Rio, it now appears, is the same with that of Sefström.

In the course of last winter my attention was directed to a mineral from Wanlockhead, which I obtained from Mr Rose

as an arseniate of lead, to some varieties of which mineral it has a strong resemblance. On analysis, however, it proved different; and though I failed in obtaining pure oxide of chromium, yet I suspected at first that it contained a considerable quantity of chromic acid. Repeated attempts, however, to obtain a *pure* oxide having failed, and many properties manifesting themselves which chromium does not exhibit, I came at length to the conclusion that it contained a new metallic substance. This substance I was engaged in examining when the letter of Berzelius in the *Annales de Chimie*, came to hand, and showed me that my new metal was the Vanadium of Sefström. My stock of the mineral was very small, and it was from a portion of it, weighing only seven grains, that I prepared the compounds of Vanadium which I soon after exhibited to the Royal Society of Edinburgh.

It is a matter of regret that both the prior discoverers should have distinguished this metal by names so unwieldy. That of Sefström, though less classical, is the more manageable of the two, yet it is time that the northern fashion of naming metals after the barbarous deities of their forefathers should be exploded.

The Vanadate of lead, or minerals containing the metal in combination with lead, I have met with in two forms differing very much from each other.

I.—The more common has much resemblance externally to some arseniates—approaches also in colour to that of some phosphates, and molybdates of lead. It is opaque, varying in colour from a straw yellow to a reddish-brown, and is generally dull, though in one beautiful wax-yellow specimen it has very considerable lustre. The lustre of fractured surfaces is resinous. It is scratched by the knife, giving a white streak; is brittle, and has a conchoidal fracture. The specific gravity in two specimens was 6.99, and 7.23 respectively.

It occurs most abundantly in small mamillæ, from a minute microscopic size to that of a large pin head, sprinkled over a surface of calamine. Occasionally it forms a thin coating upon the calamine. In the finer specimens the individuals are larger, and exhibit groups of six-sided prisms, disposed sometimes in a dendritical form, but more commonly in rounded pisiform

masses, in which the crystalline forms are more or less distinct and which appear to be only a developement of the minute mamillæ. Isolated and perfect crystals are rare.

Heated to redness in a platinum crucible it decrepitates and assumes an orange red colour, changing when it cools to a beautiful pale yellow. Before the blow-pipe in a pair of forceps it fuses, and on cooling retains its yellow colour. If kept some time in fusion, however, it is changed into a steel grey porous mass, which upon charcoal gives immediately globules of lead. Alone on charcoal it fuses readily, exhales the odour of arsenic, gives globules of lead, and leaves after heating in the inner flame a steel grey *very fusible* slag, which exhibits the reactions of chromium. The fusibility is characteristic of Vanadium.

The sulphuric and muriatic acids decompose the mineral, giving green solutions of the oxide, and forming sulphate or chloride of lead. With nitric acid it forms a beautiful yellow solution. When the latter acid acts upon it, the oxide of lead is first dissolved, leaving the fragments covered with a beautiful red coating of the Vanadic acid, which separates occasionally in the form of scales, and is afterwards taken into solution.

II.—The second form of this mineral can hardly be distinguished in external characters from earthy porous peroxide of manganese. It occurs amorphous and in small rounded forms, often powdering the calamine with a thin black coating, and at times scattered about in the cavities. It is steel grey, and porous, as if it had been subjected to heat. Before the blow-pipe it exhibits the same phenomena as the former variety.

This mineral has hitherto been found only in one mine at Wanlockhead, and only in one spot about six fathoms in length, where the vein had been subjected to a violent disruption. The mine has been unwrought for the last five or six years, and the only specimens to be met with are among the rubbish of the old workings. A supply of these specimens may be obtained from Mr Rose, mineral-dealer, South Bridge.

I have not yet been able to satisfy myself as to the *precise* composition of these minerals. I hoped to have given a correct analysis in the present, but want of time obliges me to defer it to a future number.

ART. XX.—*On some properties in Achromatic Object-glasses applicable to the improvement of the Microscope.* By JOSEPH JACKSON LISTER, Esq.

THE improvement of the achromatic compound microscope having been an occasional object of my leisure for several years past, my attention has in consequence been attracted to some properties of object-glasses of short focus and large aperture, which, so far as I am aware, have not been before noticed, and which, I flatter myself, may be applied to increase its powers and the ease of its manufacture.

In offering these, accompanied by some other miscellaneous remarks, it may be explanatory to introduce them by a short notice of the several achromatic object-glasses for the microscope which have originated apparently independent of each other within a few years past.

The first produced in England were the triple ones constructed in 1824 and 1825 by Tulley, who had been incited to the undertaking by Dr Goring: of these an account is already before the public. Tulley has since adopted for his triple object-glass to be used singly the focal length of 0.9 inch, applying another of not quite 0.5 inch focus before it when high magnifying power is required; and he obtains with these an image of great sharpness and perfection. His first glasses have all the merit of a new invention, having been executed without the knowledge that anything of the kind previously existed, though other achromatic glasses had before been made in France by Selligie, by Fraunhofer at Munich, and by Amici at Modena.

The glasses of Selligie's microscope, of which a report was made by M. Fresnel to the Royal Academy of Sciences in 1824, were composed of a plano-concave lens of flint-glass, and a double convex of crown or plate, with their inner curves cemented together. Four of these, of from $1\frac{1}{2}$ to $1\frac{7}{8}$ inch each in focal length, were made to screw before each other, so as to be used together or alone in the manner long practised with single lenses.

The chromatic aberration was thus in a considerable degree

corrected, but the glasses were fixed in their cells with the convex side foremost, which is their worst position ; and the spherical error was in consequence enormous, showing itself even through the contracted opening, to which it was necessary on that account to limit them.

Yet, inferior as was the instrument of Selligie, the happy idea of combining achromatic object-glasses, now generally adopted, and to which their present superiority is owing, seems to have occurred to no one else till put in practice by him ; and the very simple structure of his glasses will be shortly seen not to be incompatible with the finest microscopic vision.

Chevalier of Paris having manufactured some of these instruments, appears to have observed the great error in the position of Selligie's glasses ; he retained their construction, but turned their plane sides foremost ; and making them of shorter focal length, and more correctly achromatic, produced in 1825 a microscope far superior to the former. His deepest glasses are not more than 0.4 inch in focal length, and two of these were united in his earlier instruments for his highest power ; but this was the only combination retained in them, and all his glasses were restricted to apertures too small to show difficult test objects.

He has since increased the number of his glasses to be used together, and otherwise improved their performance ; but if I may judge from microscopes of his which I have recently seen in this country, he does not yet derive from the construction that he has adopted, all the advantage which it may afford.

I am unacquainted with the date of the first production of Fraunhofer's glasses ; they resemble the French in having their flint lens plano-concave, but they are not cemented, and the inner surfaces are not in contact, each object-glass being adapted by the curves of its convex lens for being used alone. My friend Mr Brown has kindly lent me a series of five glasses of this description, purchased by him at Munich a few months ago from the establishment of Utzschneider and Fraunhofer, which range from 1.8 to 0.43 inch in focal length, and are of excellent workmanship : they screw before one another in the manner of Selligie's. It appears from an account of the microscope of those artists, printed in 1829, that it is only lately

the glasses have been combined together, and that of the shortest focus added to the former series of four: and also that they are intended to be used in the order of their focal lengths, the shorter towards the object. Each of these glasses singly admits but a small pencil of from 8° to 15° of light, and when so used, their defining power is necessarily not very great; but their combinations have much more, and the different effects of these will be again adverted to.

The eminent professor of Modena, besides inventing his well-known reflecting microscope, was engaged about the year 1815 with achromatic object-glasses; but as they did not equal his reflector, he laid the work aside, till he was induced to resume it in 1824, from reading the report already mentioned on the microscope of Selligue: from this Amici took the thought of combining his object-glasses, and pursued it with great success, making them double, with the curves of the two lenses of each planned for the place it is to occupy, and for obtaining a good image either with the back glass alone, or with a second or a third in front.

He brought with him, when he visited London in 1827, some glasses of this description of very fine performance; and I have been informed by him that he has since executed a combination of “2.7 lines in focal length and 2.7 lines in aperture,” which considerably excels them.

The glasses which have been enumerated possess very different degrees of merit, chiefly dependent on the extent to which they are divested of chromatic and spherical aberration, and particularly, in connection with this, on the focal angle of their aperture; for it has been well established, that a large pencil from the object is absolutely essential to that brilliancy and distinctness of image which characterize a fine achromatic.

When the rays received by the most perfect object-glass from any indefinitely small bright portion of an object in the centre of its field are brought together at its conjugate focus, the image formed by them, though it appears a sharply defined point if moderately magnified, is really a spot or small circle, and will show as such if the microscope is sufficiently overcharged with power in the eye-glass. These circles bear a considerable analogy to the spurious disks of stars; and like them

they will be found to be much enlarged by diminishing the aperture of the object-glass.

They are enlarged also, without contracting the aperture of the glass, by increasing the intensity of the illumination, whereas by darkening the object beyond a certain point they may be rendered ill defined, and be at length dissolved.

These peculiarities are most observable on some opaque objects, (the reflection from a very small microscopic globule of quicksilver offers perhaps the best example,) but the same effects are produced on the light received from transparent ones; and the consequent blunting and mingling together of their minute details when the object-glass admits but a small pencil of light, gives rise to various fallacious appearances. One of the most remarkable is the spottiness which some surfaces assume, not unfrequently so much resembling small globules as to have been mistaken for them; an optical illusion having thus been the basis of some ingenious speculations on organic matter.

Such appearances have little place with the finer achromatics, the large angle of whose pencil and its accurate correction enable us to magnify their image greatly, still discovering something new in our objects, before we are checked by the circles of diffusion of the effective rays; but these at last, whether proceeding from the causes mentioned or from others to be hereafter noticed, form in every microscope the boundary to defining power, except where faulty materials or workmanship give it an earlier limit.

It is the marginal rays which contribute especially to render visible close and delicate lines, such as those on the scales of lepidopterous insects, and some of the most difficult of these are even best seen when the central light is intercepted.—(See Note A.)

A glass that is far from correct in its figure will sometimes show lines of this description sharply, while the outline of the scale is indistinct, and the contrary; but one in which both aberrations are destroyed should give the outline and the lines distinct together. Even some good glasses, however, have a defect, against which we should be on our guard; that in cer-

tain directions of the light they are liable to show lines on an object which do not really exist.

It is observable, that, provided the aperture of the glass remains open, the central pencil of light admitted behind many transparent objects may be limited to a very small one without greatly impairing their sharpness; parallel lines or spots spread closely over a flat surface, often remaining plainly visible in this case, which at a far less amount of contraction by a stop behind the object-glass cannot by any management be made to appear. The reason of this seems to be, that both reflection and refraction of a part of the rays take place at such objects, by which the pencil is spread out on leaving them to a much increased angle in its progress to the glass.

The relation between the aperture of microscopic object-glasses, even of the same focal length, and the pencil of light admitted by them, will vary much, according to differences in their thickness, their combination, &c.; and as aperture is valuable only in proportion to the pencil it admits, the latter would seem to be the circumstance the more deserving attention of the two. It is so often erroneously estimated, that I will mention a simple mode of ascertaining it, which will be found pretty accurate.

Fix a piece of paper on a table, and on it place the microscope with its body horizontal, and one of the eye-pieces on; set a candle on a level with it a few yards distant; then having directed the body of the instrument so far on one side of the candle, as that the light from it shall bisect the field vertically, leaving half of it dark, trace on the paper a line corresponding to the side of one of the legs. Now, taking the focus of the object-glass as a pivot, turn the microscope horizontally to the other side of the candle, till the opposite half of the field only is illuminated, and mark again on the paper the position of the side of the leg. The measure of the angle traversed, shown by the two lines, is that of the pencil of light.

In the remarks which follow, the term correction is used to imply the effect produced by the denser concave lens of a compound object-glass upon the aberration of its convex. Thus, as in a simple convex lens, the rays which pass through it near the circumference, have their foci shorter than the more cen-

tral rays, and the colours of the violet side of the spectrum into which each ray is refracted have also their foci shorter than those of the red side ; if either of these errors is but partially removed by the concave lens, the glass is said to be under-corrected as to that aberration, and over-corrected if the opposite error is produced by it.

A large focal pencil free from all aberration is evidently the great requisite for the object-glass of the compound microscope ; a second point desirable to be attained is, that the field should be flat and well defined throughout ; and a third, that the light admitted should as much as possible be only such as goes to form the picture, and should not be intercepted or diffused over the field by too many reflections.

The prominent obstacle to obtaining a sufficient pencil for high powers by one object-glass of large aperture and deep curves, is that the correction for the spherical figure by the concave lens is greater for the rays of the circumference than its due proportion to that for the more central ones ; so that when such a glass is corrected for the mean of the pencil, if we suppose its disk divided into a central space and three rings surrounding it, the rays which pass through the central space, and those of the second ring from it, will arrive at their focus when those of the first ring will have just crossed the axis, and those of the marginal ring will not quite have reached it. The injury resulting to the defining power is in similar glasses inversely as the squares of their focal lengths, as far as regards this cause of error, as well as those which arise from incorrigible colour and defects of workmanship. The effects upon the pencil which have been before described, must not be included under the same law. This excess of correction in the marginal rays increases after a certain point so rapidly with a small enlargement of the aperture of the glass, as soon to prescribe a limit, beyond which it cannot be carried without injury to the picture.

With glasses of more contracted aperture, and at the several surfaces of which the marginal refraction is moderate, the effect alluded to is comparatively inconsiderable ; and consequently, by dividing the refraction among two or more such glasses corrected for the rays that pass through them, the pen-

cil received may be enlarged without impediment, and the light and distinctness greatly increased, thus constituting the important advantage of combination.

The triple object-glass is thus very superior to a double one when each is used singly, and the union of two triple ones has been already proved in England to be eminently effective. Tulley's 0.9 inch glass singly admits a pencil of near 20° , and his combination, one of 38° .

These triple glasses and the double ones of Professor Amici are adapted by the form of their curves each to its respective place; but the foreign double glasses which have their flint lens plano-concave, and particularly those of Utzschneider before-mentioned, are made much on one model, and intended to be each good alone. It might seem but reasonable to infer from this, that they would be unfit to be combined: and accordingly when screwed together, most of the numerous practicable changes of his series of five glasses, of which as many as four may be united for use, have much indistinctness from spherical error; and this is I think the case with all those combinations which the maker contemplated. Some peculiarities, however, observed two years ago in Chevalier's object-glasses, led me to undertake a close examination of these, which were liberally placed at my disposal for the purpose, in the hope of discovering the cause for a discrepancy which appeared in their effects.

I found that with a part of the combinations, the image of any bright point that was at some distance from the centre of the field had a faint light or coma stretching outwards from it; with others the coma was as much inwards.

The spherical aberration was in general much over-corrected, but in some triple and quadruple combinations the opposite error showed itself; and out of the whole number one triple and one quadruple were remarkably beautiful and distinct.

The result of this investigation was to disclose or confirm to me the existence of some properties of the double object-glass, which have not I believe been hitherto recorded, and which it is now my purpose to describe only in connection with the subject before us,—the improvement of the microscope.

With this in view I would premise, that the plano-concave

form for the correcting flint lens, which was probably adopted at first for its simplicity, has in that quality a strong recommendation ; particularly as it obviates the danger of error which otherwise exists in centering the two curves, and thereby admits of correct workmanship for a shorter focus. To cement together also the two lenses of the glass, diminishes by very nearly half, the loss of light from reflection, which is considerable at the numerous surfaces of a combination. I have thought the clearness of the field and brightness of the picture evidently increased by doing this ; it prevents any dewiness or vegetation from forming on the inner surfaces ; and I see no disadvantage to be anticipated from it, if they are of identical curves and pressed closely together, and the cementing medium permanently homogeneous.

These two conditions then, that the flint lens shall be plano-concave, and that it shall be joined by some cement to the convex, seem desirable to be taken as a basis for the microscopic object-glass, provided they can be reconciled with the destruction of the spherical and chromatic aberrations of a large pencil.

Now, in every such glass that has been tried by me, which has had its correcting lens of either Swiss or English flint-glass, with a double convex of plate, and has been made achromatic by the form given to the outer curve of the convex, the proportion has been such between the refractive and dispersive powers of its lenses, that its figure has been correct for rays issuing from some point in its axis not far from its principal focus on its plane side, and either tending to a conjugate focus within the tube of a microscope, or emerging nearly parallel.

Let *ab*, Fig. 4, be supposed such an object-glass, and let it be roughly considered as a plano-convex lens, with a curve *acb* running through it, at which the spherical and chromatic errors are corrected, which are generated at the two outer surfaces ; and let the glass be thus free from aberration for rays *fde* *g* issuing from the radiant point *f* ; *hc* being a perpendicular to the convex surface, and *id* to the plane one. Under these circumstances, the angle of emergence *geh* much exceeds that of incidence *fdi*, being probably almost three times as great.

If the radiant is now made to approach the glass so that the course of the ray $fdeg$ shall be more divergent from the axis, as the angles of incidence and emergence become more nearly equal to each other, the spherical aberration produced by the two will be found to bear a less proportion to the opposing error of the single correcting curve acb ; for such a focus therefore the rays will be over-corrected.

But if f still approaches the glass, the angle of incidence continues to increase with the increasing divergence of the ray, till it will exceed that of emergence, which has in the meanwhile been diminishing, and at length the spherical error produced by them will recover its original proportion to the opposite error of the curve of correction. When f has reached this point f'' , (at which the angle of incidence does not exceed that of emergence so much as it had at first come short of it,) the rays again pass the glass free from spherical aberration.

If f be carried from hence towards the glass, or outwards from its original place, the angle of incidence in the former case, or of emergence in the latter, becomes disproportionately effective; and either way the aberration exceeds the correction.

These facts have been established by careful experiment; they accord with every appearance in such combinations of the plano-convex glasses as have come under my notice, and may I believe be extended to this rule;—that in general an achromatic object-glass, of which the inner surfaces are in contact, or nearly so, will have on one side of it two foci in its axis, for the rays proceeding from which it will be truly corrected at a moderate aperture; that for the space between these two points, its spherical aberration will be over-corrected, and beyond them either way under-corrected.

I am not aware that an exception is to be made for any quality of glass or curves that are likely to be used for the microscope: but I apprehend a case may occur, if the flint glass is convexo-concave and the convex lens united to its concave side, that neither of the aplantic pencils may converge after traversing the glass, and that their foci for a radiant may be on opposite sides of it, the principle however of the two foci remaining unaltered. See Fig. 5.

To try this principle under a great change of circumstances,

and to prove in what manner it was applicable to another most simple form of the object-glass, having made a magnified tracing of the curves of one of Utzschneider's, and drawn a ray through it from its longer aplanatic focus, which was ascertained after cementing the lenses; I laid down a figure of the flint lens inverted, and by means of the angles of the other diagram projected a plano-convex π , Fig. 5, to be joined to its flat side. The new glass proved as achromatic as the original, though the single radius of the plano-convex was more than one-third longer than corresponded with the curves of the former double convex lens: the two aplanatic foci showed themselves as before; the longer, however, not in the place indicated by the ray drawn: but at a point F, about two-thirds more distant from the glass, and from which both surfaces of the flint lens bent the pencil outwards; the shorter focus F'' too becoming about half the length of the other, and the angle of incidence of its rays not equalling that of their emergence.

The longer aplanatic focus may be found when one of the plano-convex object-glasses is placed in a microscope, by shortening the tube if the glass shows over-correction, if under-correction by lengthening it, or by bringing the rays together should they be parallel or divergent, by a very small good telescope. The shorter focus is got at by sliding the glass before another of sufficient length and large aperture that is finely corrected, and bringing it forwards till it gives the reflexion of a bright point from a globule of quicksilver, sharp and free from mist, when the distance can be taken between the glass and the object.

The longer focus is the place at which to ascertain the utmost aperture that may be given to the glass, and where, in the absence of spherical error, its exact state of correction as to colour is seen most distinctly.

The correction of the chromatic aberration, like that of the spherical, tends to excess in the marginal rays; so that if a glass, which is achromatic with a moderate aperture, has its cell opened wider, the circle of rays thus added to the pencil will be rather over-corrected as to colour.

The same tendency to over-correction is produced, if, without varying the aperture, the divergence of the incident rays

is much augmented; as in an object-glass placed in front of another: but, generally, in this position a part only of its aperture comes into use; so that the two properties mentioned neutralize each other, and its chromatic state remains unaltered. If, for example, the outstanding colours were observed at the longer focus to be green and claret, which show that the nearest practicable approach is made to the union of the spectrum, they usually continue nearly the same for the whole space between the foci, and for some distance beyond them either way.

The places of these two foci and their proportions to each other, depend on a variety of circumstances. In several object-glasses that I have had made for trial,—plano-convex, with their inner surfaces cemented, their diameters the radius of the flint lens, and their colour pretty well corrected—those composed of dense flint and light plate have had the rays from the longer focus emerging nearly parallel; and this focus has been not quite three times the distance of the shorter from the glass: with English flint the rays have had more convergence, and the shorter focus has borne a rather less proportion to the longer.

If the inner surfaces are not cemented, a striking effect is produced by minute differences in their curves. It may give some idea of this, that in a glass of which almost the whole disk was covered with colour from contact of the lenses, the addition of a film of varnish so thin that this colour was not destroyed by it, caused a sensible change in the spherical correction.

I have found that whatever extended the longer aplanatic focus, and increased the convergence of its rays, diminished the relative length of the shorter. Thus by turning to the concave lens the flatter instead of the deeper side of a convex lens, whose radii were to each other as 31 to 35, the pencil of the longer aplanatic focus, from being greatly divergent, was brought to converge at a very small distance behind the glass; and the length of the shorter focus which had been one-half that of the longer, became but one-sixth of it.

The direction of the aplanatic pencils appears to be scarce-

ly affected by differences in the thickness of glasses, if their state as to colour is the same.

One other property of the double object-glass remains to be mentioned ; which is, that when the longer aplanatic focus is used, the marginal rays of a pencil not coincident with the axis of the glass are distorted, so that a coma is thrown outwards ; while the contrary effect of a coma directed towards the centre of the field is produced by the rays from the shorter focus. These peculiarities of the coma seem inseparable attendants on the two foci, and are as conspicuous in the achromatic meniscus, as in the plano-convex object-glass.

(To be concluded in next Number.)

ART. XXI.—*Notice respecting the proposed Scientific Meeting at York on Monday the 26th of September.*

SOME months ago it occurred to the Editor of this work that the general interests of science might be greatly promoted by the establishment of a Society of British Cultivators of Science, which should meet annually in some central town in England. He accordingly corresponded on the subject with several influential individuals, and found that there was a general desire for the organization of such a society. As York possessed peculiar advantages for the first place of meeting, from its local position in reference to the three capitals of the kingdom,—from its having a Philosophical Society, containing many distinguished members,—and from its having a museum and other apartments, suited for the meetings of the society, he addressed himself to Mr Phillips, the Secretary to the Yorkshire Philosophical Society, in order to ascertain how such a meeting would be viewed by that society and by the principal authorities in York. Mr Phillips lost no time in obtaining the desired information ; and having made application to the Mayor of York, (the Right Honourable Lord Dundas, we believe,) he was able to report that the authorities of York entered heartily into the plan, and that the Philosophical Society would willingly charge themselves with any preliminary arrangements which might be necessary.

Upon receiving Mr Phillips's letter, I transmitted it to John Robison, Esq. who had kindly agreed to act as interim secretary,

and who, with the assistance of J. F. W. Johnston, Esq. who attended the last meeting of German Naturalists at Hamburg, proposed to draw up a set of regulations or laws to be submitted to the society at its first meeting.

Under these circumstances, I drew up the notice of the meeting, which was published in the last number of this *Journal*, and which has been widely circulated by the periodical press.

These steps having been taken previous to the dissolution of Parliament, it was conceived by some individuals, who were extremely anxious about the success of the plan, that the election might interfere with the meetings of the society, and that it might be more prudent to delay its establishment to another year. As the political agitation of the country has entirely subsided, there can be no sufficient reason for such a delay, and it has accordingly been agreed upon to hold the first meeting of the society at York on *Monday the 26th September*.

The following Regulations, most of which are adopted by the German association, are suggested to the consideration of the Yorkshire Philosophical Society, who ought to be prepared with a Code of Laws to be submitted to the Society at their first meeting. The rules now suggested will be useful in enabling those gentlemen who mean to attend, and who will be entitled to act as Members, to transmit (post paid) any observations or suggestions on this subject, to R. Phillips, Esq. Secretary to the Philosophical Society, York.

1. The Society shall be called the Society of the British Cultivators of Science.

2. The principal objects will be to promote personal intercourse among scientific individuals, and to adopt every means for advancing the interests of science in every part of the world, by promoting scientific inquiries, and procuring assistance to men of science in the prosecution of their researches.

3. That the Society shall consist of those only who have written upon scientific subjects, or who have been the zealous patrons of science.

4. The Society shall be managed by a president and a secretary, resident in the place of meeting for the time being.

5. The Society shall meet annually on the ——— and shall deliberate with open doors.

6. The Society shall hold its meetings at different towns,

where sufficient accommodation can be obtained, and the place of meeting for the next year shall be fixed by the Society before the termination of its sittings.

7. The Society shall hold no property ; but if books, &c. are presented to it, they shall be lodged in the museum of the town where the Society last met.

8. A record of the proceedings of the Society shall be kept by the secretary.

9. The expences of the Society shall be defrayed by the contributions of the members present.

The following printed notice is now in circulation ; and it is requested that the Editors of newspapers will have the goodness to give publicity to the views of the Society.

“ A strong desire having been expressed that a General Meeting of friends of Science should take place annually in some central town of England, with the view of promoting unrestrained communication of scientific opinions and discoveries, a Committee of the principal Scientific Societies of London, Edinburgh, &c. have fixed on the City of York as a most desirable place for the FIRST MEETING ;—to commence on *Monday* the 26th of *September*, a period of the year which has been ascertained to be most convenient for the parties interested, the Meeting to be continued during as many days as may be deemed expedient.

“ Any friends of Science in Great Britain or in other parts of Europe, who may wish to attend this Meeting, are requested to send a letter (post paid) to the Secretary of the Yorkshire Philosophical Society, York, in order that adequate preparation and accommodation may be secured. It is proposed that the visitors shall assemble in the Museum and apartments of the Yorkshire Philosophical Society, to receive memoirs and communications, and that they shall dine together daily.

“ Persons arriving in York on or before the 26th of September will on application to the Porter of the Museum receive information as to the hours and places of Meeting.

“ Foreigners who may honour this Meeting with their presence will find every accommodation prepared for them.”

ART. XXII.—*Observations on Fossil Vegetables, accompanied by Representations of their Internal Structure as seen through the Microscope*. By HENRY WITHAM, Esq. of Lartington, Fellow of the Geological Society of London ; of the Royal and Wernerian Societies of Edinburgh, &c.

THE following extracts which we make are from a splendid work which bears the above title, by Mr Witham. The importance of the volume is not little, as we are furnished by it with the ready means of examining the character and internal structure of plants of different geological epochs, in a mode hitherto little known to the class of naturalists to whose use this volume is chiefly designed.

“My investigations,” observes Mr Witham, “have led me to believe, that plants of the Phanerogamic class are much more abundant in our coal-fields and mountain limestone groups, than has generally been supposed. The great opacity and peculiar mineralogical arrangements of these fossil plants, have presented obstacles to the examination of their intimate structure, which have induced naturalists to rest contented with the distinctive characters afforded by their external forms ; and in many instances, these forms are obviously too much altered, to permit us to refer the objects in question, with perfect satisfaction, to any natural family. But a method has lately been discovered, by which the stems or branches may be sliced, and afterwards reduced to such a degree of thinness as to permit us to inspect the most minute remains of organic texture. The unexpected result thus obtained, has enabled me to examine numerous varieties of structure in fossil plants ; and I feel confident, that I should be rendering a service to science, by presenting to the public representations of some of these varieties, accompanied by others of those recent plants to which they seem decidedly to approximate.

“According to the opinion of those who have most successfully cultivated geological botany, the essential character of the first vegetation of the globe consisted in the great development and numerical preponderance of the vascular cryptogamic plants ; and the great size of the Ferns, Lycopodiaceæ, and Equisetaceæ, imbedded in our deposits, authorize us to presume, that, during that period, circumstances calculated to

favour the development of these plants had prevailed in a very high degree. May we not now go a little farther, and, from the recent discoveries, suppose that heat and moisture had an equal effect upon plants of different genera from those of the vascular cryptogamic? When we discover stems of fossil plants thirty, sixty, and even seventy feet long—when each torrent by its violence uncovers so many relics of ancient times—nay, whenever, by the busy ingenuity of man, the lower as well as the higher sedimentary desposits are interfered with or disturbed, some previously unobserved fossil is discovered, it is but fair to conclude that the same causes have equally affected them.

“By the investigations of many of my friends, and by my own observations, I am induced (as already noticed) to think that plants of the phanerogamic class will be found to occur in greater abundance in the earlier sedimentary deposits than was before believed. The day is now arrived, when doubts and difficulties as to the class and family to which they are to be referred, must give way under the microscopic examination of the internal structure of these fossil plants; and I am happy in having it in my power to contribute in some degree to the elucidation of this interesting subject.”

After these and other prefatory observations, Mr Witham states the arrangement of his volume, which is, “1st, To offer some remarks on the Vegetation of the first period of the Ancient World, that is, from the first deposit of the transition series to the top of the coal-field; 2dly, To present an account of Fossil Vegetables found at Lennel Braes and Allanbank Mill, in Berwickshire; 3dly, To exhibit representations of the organic texture, as discovered by the microscope, of several fossil plants of the Coal formation, Mountain Limestone group, and of the Lias, together with corresponding representations of recent plants, of similar or analogous structure, and comparative views of other fossil and recent vegetables, accompanied with descriptive references; and, 4thly, To conclude with some general remarks on the varieties represented and described, and on the subject of fossil plants in general.”

The first and second parts of Mr Witham's divisions we shall for the present pass over, not, however, without remarking the valuable facts which they record. The section of the work

more immediately connected with the object of the publication is the third, in which the skilful author thus unfolds his plan,

“Hitherto, the attention of geologists has been exclusively confined to the external forms of fossil plants; and these forms, illustrated by reference to living species, and to vegetable anatomy, have afforded characters by which numerous species and genera may be distinguished with accuracy. But the supposed destruction of the internal structure of most fossil plants, and the difficulty of applying the microscope to those which evidently retain it, have hitherto prevented our becoming acquainted with the organization of these plants. Many fossil vegetables are converted into a mass of carbonaceous matter: others are filled with up sand, and other substances, the external part, or cortex, alone remaining; but it has been found that many retain their original structure, the interstices being filled up by calcareous or siliceous crystallizations. A method has lately been discovered, by which the organization of the latter may be satisfactorily examined.”

A number of beautiful plates, amounting to six in number, then follow, containing “representations of the internal structure of Recent and Fossil Plants belonging to the Gymnospermous Phanerogamic, Monocotyledonous Phanerogamic, and Dicotyledonous Phanerogamic Classes. The portions represented are of very thin slices, viewed by transmitted light and magnified about fifty-five times.

Into the details of description connected with the internal structures thus described, we are precluded from entering; nor, indeed, could we well make them intelligible to our readers without the aid of Mr Witham's valuable plates. We shall therefore hasten to the last section, in which the author's conclusions are explained.

“The first general remark which I have to offer respecting the fossil vegetables figured in the Plates, is, that their concentric layers present the same irregularity as those of our recent plants. An inference to be made from this circumstance is, that the climate which existed at the epochs when these vegetables grew, resembled ours in the irregularity of its successive summers. If, at the present day, a warm and moist summer produces a broader annual layer of wood, than a cold or dry one, and if fossil plants exhibit such appearances as we

refer in recent plants to a diversity of summers, then it is reasonable to suppose that a similar diversity formerly prevailed.

“ The Coniferæ of the coal-formation and mountain limestone group, have few and slight appearances of the lines by which the annual layers are separated. The trees of our present tropical regions have also few and slight appearances of these lines. Therefore, at the epochs of these formations, the changes of season were probably as little marked as they are in our tropical regions.

“ Again, the condensation observed towards the outer margin of each woody layer of the trees of our cold and temperate climates, and which is attributed to the increasing cold of the latter part of the autumnal season, is decidedly observable in the Coniferæ of the lias ; as in Figs. 3, and 4, of the lower part of Plate IV., and in Figs. 1, 2, and 3, of Plate V. Therefore, at the epoch when the trees of the lias grew, there was a cold season as now.

“ Between the monocotyledonous fossil and recent species which I have figured, no comparison can be instituted, as they are not of the same species ; but it may be remarked generally, that, so far as I have examined these plants, the recent species have smaller cells and vessels than the fossil.

“ The same remark is to be made of the dicotyledonous woods.”

Mr Witham then remarks, that some of the recent Coniferæ which he has figured, are of climates similar to that in which the fossil Coniferæ are figured ; yet that even here no accurate comparison can be instituted, as we cannot decide upon the genera, much less upon the species of the fossil plants ; but that in general the cells or tubes of the latter were in almost every case much larger than those of the former. “ Our observations, however, on this subject,” adds the author, “ are not sufficiently numerous or correct, to authorize any inferences as to the comparative vigour of the vegetation of the different epochs, although they are sufficient, along with other circumstances, to render it not improbable that the temperature was higher at the periods when the fossil vegetables grew than it is now.

“ The general colour of the fossil vegetables, retaining their organic texture, which occur in our mountain limestone groups,

coal-fields, and lias deposits, is brown, of various tints, more commonly wood-brown, frequently umber, and sometimes greyish or blackish-brown. These fossils are all more or less calcareous, and the veins by which they are frequently intersected, are generally of calcareous spar. Those fossils, on the contrary, which, occurring in the same formations, are destitute of organic texture, have their interior filled with substances analogous to those of which the strata containing them are composed.

“ Silicified fossils occur more abundantly in the superior formations, and are generally of a lighter colour. The calcedonic monocotyledonous and dicotyledonous fossils of the West Indies vary in colour from yellowish-white to reddish-brown, or even bright red, but seldom or never assume tints so dark as those of our inferior deposits.

“ With respect to the fossil vegetables of which I have presented figures, I may here again remark, that, notwithstanding the want of well-defined concentric layers in those of the coal-formation and mountain limestone group, no doubt remains with me as to their being Coniferæ. Should it be shown, by future investigations, that recent plants of other classes present a similarity of structure, the case will become different; but until then, it will remain established that these fossil plants come nearer to the structure of the Coniferæ than to that of any other tribe. As to the fossils of the lias, I presume no doubt can henceforth remain in the mind of any one who may compare them with recent Coniferæ, or be satisfied with the accuracy of my representations.”

Mr Witham concludes his highly interesting work by giving details of the mode in which he prepares objects for the microscope, for the process of which he acknowledges being indebted to Mr Nicol.

“ Let a thin slice be cut off from the fossil wood, in a direction perpendicular to the length of its fibres. The slice thus obtained must be ground perfectly flat, and then polished. The polished surface is to be cemented to a piece of plate or mirror glass, a little larger than itself, and this may be done by means of Canada balsam. A thin layer of that substance must be applied to the polished surface of the slice, and also to one side of the glass. The slice and the glass are now to be laid on any thin plate of metal, as a common fire-shovel,

and gradually heated over a slow fire, with a view to concentrate the balsam. In performing this operation, it will be requisite to prevent the heat from becoming so great as to throw the balsam into a state of ebullition; for, if air-bubbles be once formed in it, it will be difficult to remove them, and if they are not removed, they will prevent the complete adhesion of the two surfaces when applied to each other. The heat of the shovel should never become so great that the fingers may not be held in contact with it, without inconvenience, for a few seconds. With every precaution, some few air-bubbles will sometimes make their appearance, but these may be removed by a small piece of wood tapering to a point. When the balsam is thought to be sufficiently concentrated, and all air-bubbles completely removed, the slice and the glass may be taken from the shovel, and applied to each other. A slight degree of pressure will be necessary to expel the superabundant balsam, and this will be facilitated by gently sliding the one on the other. By this kind of motion, any air that might have got entangled in the balsam, when the surfaces were brought into contact, will also be removed.

“ When the whole is cooled down to the temperature of the air, and the balsam becomes solid, that part of the balsam adhering to the surface of the glass surrounding the slice, should be removed by the point of a pen-knife; and it may be right to remark, that, in this operation, it will at once be seen whether the balsam has undergone the requisite concentration. If, for instance, it has entirely lost its sectility, and starts off in flakes before the knife, it will be found that the slice and the glass will cohere so firmly, that, in the subsequent grinding, there will be no risk of their separating from each other. If the balsam has not been sufficiently concentrated, it will slide before the knife, and, in that case, the two bodies will not adhere with sufficient firmness. A very few trials, however, will enable any one to conduct the process with success; and it may be right to add, that, if the layer of balsam applied to the two surfaces be not too thick, its due concentration may be accomplished in four or five minutes, provided the application of the heat be duly regulated.

“ The slice must now be ground down to that degree of thinness which will permit its structure to be seen by help of a

microscope. To facilitate this part of the grinding, the lapidary will find it advantageous to fix the glass in a groove made in a small piece of wood, of which half inch thick deal will answer the purpose. The groove in the wood should be a little less deep than the thickness of the glass, and the wood itself need not project more than half an inch beyond each side of the glass.

“A lapidary, by attending to the above directions, will find no difficulty in reducing any piece of petrified wood to that degree of thinness sufficient to render its structure visible; and any one, even without the aid of the mechanism employed by the lapidary, may accomplish that object by attending to the following directions.

“The position of the fibres of the wood having been ascertained, let a thin piece be chipped off by a blow of a hammer, in a direction perpendicular to the length of the fibres. Let the chip thus obtained be cemented to any small bit of wood by common lapidaries' cement (a compound of 1 part bees' wax, 1 part pitch, 4 parts rosin, 16 parts of a mixture of brick-dust and whitening,) to enable the operator to hold it firmly while the grinding is going on. That side of the chip which approaches nearest to a perpendicular to the length of the fibres, must be ground flat, by giving it a rapid circular motion with the hand, on a piece of sheet-lead lying horizontally on a table, and supplied with a little emery, size No. 1, moistened with water. When the emery ceases to act, the muddy matter remaining may be removed, and a fresh portion of emery applied; and this must be repeated until the surface of the chip has become perfectly flat. The sheet of lead must then be removed, and a piece of flat sheet copper substituted, and the surface of the chip ground as smooth as may be, by flower of emery, freed from its coarser parts. The surface may then be polished by friction with crocus or rot-stone, on a transverse section of any soft wood.

“When the polishing is finished, the chip must be detached from the wood to which it was cemented, and the polished surface cemented by Canada balsam to a piece of plate-glass, in the manner above described, and then ground thin, and polished as before.”

ART. XXIII.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, kept at Canaan Cottage. By ALEX. ADIE, Esq. F. R. S. Edin.
 The Observations contained in the following Register were made at Canaan Cottage, the residence of Mr Adie, by means of very nice instruments, constructed by himself. Canaan Cottage is situated about 1½ mile to the south of Edinburgh Castle, about 3 miles from the sea at Leith, and about ¼ of a mile N. of the west end of Blackford Hill. The ridge of Braid Hills is about 1 mile to the south, and the Pentland Hills, about 4 miles to the west of south. The height of the instruments is 300 feet above high water-mark at Leith. The morning and evening observations were made about 10 A.M. and 10 P.M.

MARCH 1831.

APRIL 1831.

MAY 1831.

Day of Month.	Thermometer.			Register Therm.			Barometer.		Rain.	D. of Week.	D. of Mon.	Thermometer.			Register Therm.			Barometer.		Rain.
	Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.				Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.	
1 T.	40	51	57	51	41	57.5	29.18	29.41	.21	S.	1	40	57	58.5	59	40	58.5	50.19	50.52	.02
2 W.	43	49	47	55	51	42	29.09	29.05	.03	S.	2	41	58	59.5	56	45	53.5	50.10	50.05	.03
3 T.	45	45	40	58	50	44	29.03	29.32	.03	S.	3	40	50	55	56	43	53.5	29.90	29.02	.48
4 W.	47	47	47	54	52	45	29.30	29.16		S.	4	43	40	42.5	57	55	40	29.10	29.22	
5 T.	48	41	46	44	50	47	29.20	28.97		S.	5	47	44	45.5	52	43	52	29.15	29.00	
6 W.	44	42	41	40	51	45.5	28.86	29.11		T.	6	40	40	40	58	44	41	29.15	29.00	
7 T.	44	44	45	40	55	49	29.52	29.50		T.	7	48	47	47.5	55	51	45	29.15	29.00	
8 W.	42	45	43.5	52	48	40	29.50	29.05		F.	8	49	45	46	40	49	41.5	28.87	28.81	
9 T.	47	55	41	55	51	43	29.59	29.45		F.	9	53	45	50	57	57	47	29.11	29.21	
10 W.	47	56	41.5	52	49	50	29.52	29.47	.50	S.	10	50	47	48.5	40	56	38	29.92	29.48	.07
11 T.	47	56	41.5	52	49	50	29.18	29.19	.53	T.	11	53	45	48	40	58	49	29.72	29.50	.02
12 W.	47	47	43.5	52	47	50.5	29.12	28.90		M.	12	55	48	51.5	57	60	45.5	29.15	29.35	
13 T.	45	54	58.5	57	48	42.5	28.85	28.95	.08	W.	13	45	45	41	47	41.5	45	29.70	29.71	.20
14 W.	40	56	58	50	43	56.5	29.02	29.04	.19	T.	14	51	45	48	56	54	45	29.58	29.57	
15 T.	41	45	42	52	48	40	29.06	28.85	.25	F.	15	59	46	52.5	56	51	50	29.70	29.50	
16 W.	47	55	51	41	51	46	29.21	29.25	.16	S.	16	55	47	51	62	62	52	29.81	29.78	
17 T.	49	40	44.5	45	49	46	29.17	29.58	.17	S.	17	50	40	45	45	55	50	29.85	29.87	
18 W.	46	41	43.5	40	48	44	29.80	29.67		M.	18	50	40	45	55	58	46.5	29.88	29.84	
19 T.	46	46	46	40	48	44	29.78	29.80		W.	19	43	40	42.5	57	51	41	29.79	29.76	.01
20 W.	49	46	47.5	41	53	48.5	29.30	29.98		T.	20	44	40	42	57	41	40.5	29.60	29.70	
21 T.	49	41	46.5	42	53	47.5	30.16	30.21		F.	21	45	41	45.5	58	41	41	29.52	29.50	.04
22 W.	42	42	42	40	48	44	30.05	30.16		S.	22	45	45	45	41	40	55	29.52	29.50	
23 T.	48	55	41.5	59	49	44	30.05	30.16		S.	23	41	41	42.5	51	46	40	29.55	29.70	
24 W.	59	55	57	28	40	51	30.05	29.71		T.	24	45	41	44.5	41	47	44	29.70	29.70	
25 T.	59	57	58	54	40	57	29.67	29.55	.08	M.	25	47	45	46	43	53	48	29.75	29.60	.10
26 W.	56	42	59	52	42	57	29.26	29.15	.12	W.	26	50	46	48	44	51	47.5	29.55	29.48	
27 T.	50	41	45.5	59	55	47	29.91	29.80	.02	T.	27	49	46	47.5	46	51	48.5	29.57	29.15	.56
28 W.	48	58	43	52	54	45	29.97	30.06		F.	28	49	43	46	45	53	49	29.15	29.11	
29 T.	40	40	45.5	38	42	40	30.17	30.23		S.	29	47	42	44.5	42	47	44.5	29.09	28.99	.15
30 W.	43	42	42.5	59	46	42.5	30.27	30.37		T.	30	47	42	44	42	45	43.5	28.91	28.99	.40
31 T.	43	40	41.5	40	48	44	30.45	30.46		S.		44	44	44	42	45	43.5	28.91	28.99	
Sum,	1379	1266	1323	1125	1497	1311	915.18	915.22	1.97			1425	1281	1355	1168	1335	1360.5	885.65	885.21	1.54
Mean.	44.48	40.84	42.66	36.29	48.29	42.29	29.522	29.525				7.5	42.7	45.1	38.95	51.1	45.01	29.522	29.508	

THE
EDINBURGH
JOURNAL OF SCIENCE.

ART. I.—*Some Account of James Stirling, F. R. S.* By S. P. RIGAUD, F. R. S. Savilian Professor of Astronomy in the University of Oxford. Communicated by the Author.

JAMES STIRLING was a very eminent mathematician, but hardly any thing seems to be generally known about him; and Hutton * was able to collect very little more than the record of his death from the obituary of the *Gentleman's Magazine*. He was not, however, one of those common-place men who just come into the world “to look about us and to die;” a few additional particulars will therefore not be without an interest in themselves, and, by recalling attention to his memory, may be the means of our learning more. His family may yet be resident in his native country, and when they find that his name is still honoured as it deserves, they may, if they are able, be induced to give us further information about him.

He was the third son of Archibald Stirling of St Ninians, in the county of Stirling. On the 4th of January 1711, he was admitted a commoner of Balliol College, Oxford, and, on the 10th of the same month, he became one of Snell's exhibitors in that society. At this time he was in the fifteenth year of his age, and consequently, he must have been born in 1696. The particular day of his birth is not preserved in the University Registers, from which these dates are taken.

* *Phil. Trans. abr.* vol. vi, p. 428.

On the death of Queen Anne in 1714, there was a very large portion of the British population which was adverse to the succession of the Brunswick family ; and the events of 1715 showed that the attachment to the Stuarts was retained nowhere with greater force than in Scotland. Young Stirling participated in the feelings of his countrymen, and was tried at the Oxford Assizes in the summer of 1716, for treasonable language which he had used against George the First. The jury brought him in not guilty ; but he did not clear himself from suspicion, as there is a letter in the Bodleian Library from Archbishop Wake to Dr Charlett, (dated July 28, 1716,) in which he says, " I have had a full account of what passed at your assizes, from the sermon at St Mary's, to the stop put to the hurra that had like to have risen in the hall upon the acquittal of Sterlin. The judge did his part fairly, and the man was acquitted, yet some were never the more convinced that he was not guilty of the fact whereof he was accused. I am sorry to hear, if he were fellow of Balliol, that he is still reported to be a nonjuror, but more that I doubt some of our Scots exhibitioners, on the Bishop of Rochester's foundation, have not taken the oaths. I shall expect to be very well satisfied of these last that they have qualified themselves as the law requires, before they shall receive any more money from Bishop Warner's charity."

It was probably in consequence of these investigations that notice was taken in college of his politics ; for among Thomas Hearne's manuscript memoranda in the Bodleian we find it said :—" 1717, March 29. Mr Stirling of Balliol College, one of those turned out of their scholarships upon account of the oaths, hath the offer of a professorship in mathematics in Italy, which he hath accepted of, and is about going thither. This gentleman is printing a book in the mathematical way at the theatre." Nothing appears from which it can be ascertained whether he actually went abroad, or, if he did, where he resided, and how long he staid there ; but the book which Hearne alludes to must have been the *Lineæ Tertii Ordinis Newtonianæ*, which he published at Oxford in 1717. Montucla, by some mistake, describes it as having been printed at London in quarto ; but it was really a small octavo volume of 128 pages, with an appendix of 19 more, containing the solution of three

questions,—the curve of swiftest descent, the properties of the catenary, and a problem proposed by Leibnitz respecting the curve which would cut certain hyperbolas at right angles. The work came out by subscription; and in an old account-book of the Reverend James Pound, at that time rector of Wansted, there is an entry of his having paid, “1716, Oct. 10, by subscription for Mr Stirling’s books, * 5s.” Edmund Stone published a paper in the 41st volume *Philosophical Transactions*, “concerning two species of lines of the third order, not mentioned by Sir Isaac Newton, nor by Mr Stirling.” Stirling is said to have been very angry at this statement; and probably he was not better satisfied with Du Gua, who likewise endeavoured † to detect some partial omissions in the enumeration. It must, however, be borne in mind, that he could not have been above one-and-twenty when he published his book; and we need hardly look for a stronger testimony to its real merit, than the circumstance of its having been reprinted at Paris in 1797. The preface of this second edition speaks of it as “eximium opus cujus rarissimi libri alteram editionem jamdudum geometræ desiderabant.” The bookseller intended, if he met with encouragement, to proceed in the republication of the scarcer works of distinguished mathematicians; a design which, in many instances, would have been very desirable to have had executed, but which was not likely to be profitable to those who undertook it. It was, however, no small testimony of the high opinion entertained of this work, that it should be selected for the first on which the trial was to be made.

In 1719, Stirling contributed a paper to the 30th volume of the *Philosophical Transactions*, entitled *Methodus differentialis Newtoniana illustrata*. It was written in Latin, and in the title prefixed to it, he simply calls himself è Coll. Ball. Being a nonjuror, he of course never graduated. He afterwards published a more extensive work on the same subject, in his *Methodus differentialis sive tractatus de Summatione et interpolatione Serierum*. This came out in quarto, and Hutton says it was “first published in the year 1730, and again in

* He subscribed for two copies, as appears from the list prefixed to the book.

† Montucla, *Hist. des Mathematiques*, vol. iii. p. 69,

1764." It appears from the sale-catalogue of Dr Hutton's library, that he was in possession of both, and it is therefore extraordinary that he should have expressed himself so as to give the false impression of there having been more than one edition. It was originally printed for Strahan by Bowyer, although Nichols, in his anecdotes of that great printer, makes no mention of it. From the scarceness of the copies with 1730 in the title-page, it is fair to conclude that they did not obtain any extensive circulation ; in 1753, (a date which is still more scarce, and which is noticed by no writer on bibliography,) Manby on Ludgate Hill endeavoured to bring the work again into notice ; and lastly, in 1764, Whiston and White, in Fleet Street, became the agents to supply the demand for it. This last is the date most commonly met with ; but any one who will take the trouble of comparing the books, will find that there never was more than one edition, the copies of which have gone out to the world at several times with different title-pages.

Hutton mentions that an English translation of this work by "a Mr Francis Holliday" was published in 1749. Holliday printed a collection which he called *Syntagma Matheseos*, (8vo, Lond. 1745.) In 1745, he also began a publication, which was to be continued quarterly, under the title of *Miscellanea Curiosa Mathematica*, of which he completed one volume in nine numbers ; he likewise printed five numbers of a second volume, and began a sixth, which was never finished. The work stopped in 1755, possibly in consequence of the death of E. Cave, the publisher of it, which had taken place in the preceding year. Holliday was then master of the free grammar-school at Haughton Park, near Retford, in Nottinghamshire. In 1756, he published an easy introduction to practical gunnery, or the art of engineering, and in 1777, he printed an introduction to fluxions, having at this time become vicar of Markham and Bothamsall in Notts.

Holliday seems to have paid great attention to Stirling's writings. In the *Syntagma Matheseos* he gave a translation of the paper on the differential method which had been inserted in the *Philosophical Transactions*, and in the second volume of the *Miscellanea Curiosa Mathematica*, (p. 162,) he

says "I shall now annex two problems with their solutions in summation of series, which I hope will not displease our readers, and, when room permits, shall further illustrate Mr Stirling's book entitled *Summation and Interpolation of Infinite Series*." The translation of the whole work was originally intended for occasional insertion,* by parts, in the same periodical miscellany, but Emerson, who had assisted in the revision of it, recommended an entire and separate publication. It is valuable, not merely from its scarceness, but also from its forming a comment, in some measure from authority, upon the original text, as will be seen from the following passages which occur in the preface.

"Being unwilling to commit any mistakes that might be injurious to my readers, I had recourse to Mr Stirling himself, and he was so obliging as to give me all the assistance I needed on this occasion, and particularly his correction and amendment of example 2, † p. 132, concerning which I had some suspicion."

"In a postscript of a letter that I had the honour to receive from Mr Stirling, dated the 18th of May 1747.....I had, he says, almost forgot to give an answer to that part of your obliging letter, where you say that some eminent mathematicians had observed to you, that I had nowhere shown from a given series, how to find the equation of the successive terms: to which I answer, that the thing is self-evident, or easy in such series as I proposed to sum or interpolate; but in many kinds of series, the equation to the terms cannot be found, at least if their relation be considered in the manner I have done. For this reason, I gave no rule for finding the equations to the terms of the series which I considered, and I did not propose to solve the problem generally, because I knew that it was not to be done."

In November 1729, Stirling was elected fellow of the Royal

* Upon this plan a translation was begun of Brooke Taylor's *Methodus Incrementorum*, given with "a due medium between a paraphrase and a verbal translation." It had not, however, proceeded beyond the first ten propositions when it was stopped by the termination of the publication.

† Exp. 2. to Prop. xxvi.

Society, and he afterwards contributed two papers to the *Philosophical Transactions*. The first of them is in the 39th volume, (for 1735 and 1736;) it treats of the figure of the earth and the variation of gravity at its surface. His thoughts had been occupied on this subject for a considerable time, for he particularly mentioned it in a letter which, in 1733, he wrote to Bradley, who was his contemporary at Balliol, having entered at that same college not three months after him. The letter is dated from Tower Street, and Bradley's* answer was evidently addressed to him in London: some little interval seems to have elapsed before this answer was written; but whether he was now fixed in the metropolis, or only on an occasional visit to it, is uncertain. The other paper is contained in the 43d volume of the *Philosophical Transactions*, (for 1745,) and gives the description of a machine in which a current of air is supplied by the fall of water, so as to blow up the fire of a furnace.

It is not known that he published any other works, although he lived to a very advanced age. From the *Gentleman's Magazine*, (1770, p. 591,) we learn that he died on the 5th of December 1770, being then agent to the Scotch mining company in Leadhills. It is probable that he had been connected with that society for many years, or at least there is a circumstance that seems to indicate it; for, in his paper on the blast-furnace, which was to be urged by the means of a stream of water, he speaks of an engine sufficiently large "to smelt harder ore than any in Leadhills." If the old registers and account books of the company should be still in existence, they may enable us to ascertain whether there is any foundation for this conjecture, and may even supply us with some further particulars of the man, whose talents, no doubt, were of the greatest service to the establishment.

OXFORD, *July* 1831.

* Neither of these letters are as yet before the public; but they will appear in the collection of Bradley's miscellaneous works, which is now in the press at Oxford.

ART. II.— *On a New Analysis of Solar Light, indicating three Primary Colours, forming Coincident Spectra of equal length.* By DAVID BREWSTER, LL.D. F.R.SS. Lond. and Ed. (*Abridged from the Transactions of the Royal Society of Edinburgh.* Read 21st March 1831.)

IN submitting to the scientific world a new analysis of light, I am fully aware of the difficulties which I have to encounter. Even in physical science it is an arduous task to unsettle long established and deeply-rooted opinions, and that task becomes Herculean when these opinions are entrenched in national feeling and associated with immortal names. There are cases indeed, where the simple exhibition of new truths is sufficient to dispel errors the most deeply cherished, and the most venerable from their antiquity; but it is otherwise with doctrines which depend on a chain of reasoning where every step in the inductive process is not rigorously demonstrative; and of this we require no other proof than is to be found in the history of Newton's optical discoveries, and particularly in the opposition which they experienced from such distinguished men as Dr Hooke and Mr Huygens.

In the investigations which I am about to explain, the instrument employed is the absorbent action which different bodies exercise on different rays of white light. This principle, which science had hitherto scarcely recognized, was brought into notice by the recent discoveries respecting polarization and double refraction, and was, I believe, first employed as an instrument of analysis, in a paper printed in the *Edinburgh Transactions*, (vol. ix. p. 433.) In the experiments there described, I examined Dr Wollaston's spectrum of four colours, viz. *red, green, blue, and violet*, by means of a purplish-blue glass. This glass absorbed the *blue* rays, which, when mixed with the *yellow*, made *green*, and the *yellow* rays, which, when mixed with the *red*, made *orange*; and by insulating the *yellow* and *red*, it thus effected a perfect analysis of the *compound green* and the *compound orange*. From this experiment I drew the conclusion that *yellow* light has an independent existence in the spectrum; and that the prism is incapable of decomposing

that part of the spectrum (of four colours) which it occupies. This unequivocal result of a simple experiment at once saps the foundation of the prismatic analysis of light. Sir Isaac Newton, resting on the indications of the prism, concluded that *green* and *orange* were simple colours, and in general, "that to the same degree of refrangibility ever belonged the same colour, and to the same colour ever belonged the same degree of refrangibility;" but it is now obvious that certain *blue* and *yellow* rays, and certain *red* and *yellow* rays, have the very same refrangibility, so that, in the same medium, refrangibility is not a test of colour, nor colour a test of refrangibility.

These views were confirmed by experiments made by Mr Herschel, and printed in the same volume of the *Transactions*; but, in referring to them five years afterwards, in his *Treatise on Light*, he regarded them as liable to formidable objections. "This idea," says he (the inability of the prism to analyze light), "has been advocated by Dr Brewster, in a paper published in the *Edinburgh Philosophical Transactions*, vol. xi., and the same conclusion appears to follow from other experiments published in the same volume of that collection. According to this doctrine, the spectrum would consist of at least three distinct spectra of different colours, *red*, *yellow*, and *blue*, overlapping each other, and each having a maximum of intensity at those points where the compound spectrum has the strongest and brightest tint of that colour. It must be confessed, however, that this doctrine is not without its objections; one of the most formidable of which may be drawn from the curious affection of vision occasionally (and not very rarely) met with in certain individuals who distinguish only two colours which, (when carefully questioned and examined, by presenting to them, not the ordinary compound colours of painters, but optical tints of known composition), are generally found to be *yellow* and *blue*."

To these remarks Mr Herschel has added an illustrative figure of the spectrum, in which it is made to consist of *four* colours, *red*, *yellow*, *blue*, and *violet*; the red extending to the middle of the yellow, the yellow beginning at the extremity of the orange and terminating at the indigo, the blue beginning at the middle of the yellow, and terminating at the end of the

violet, and the violet beginning at the indigo, and terminating at the extremity of the spectrum.

I have not been able to discover what the general objections are which Mr Herschel refers to in the preceding passage: but the formidable one which he distinctly specifies will be found to have no weight. An obscure physiological fact occurring in one eye out of a million, could not, on any principle, affect the result of a legitimate induction; but even if we invest it with the character of a general fact, it will be found to be a direct argument in support of the very views which it was supposed to contradict.

These views, or the analysis of the spectrum to which they lead, may be expressed in the following propositions:

1. White light consists of *three* simple colours, *red, yellow,* and *blue*, by the mixture of which all other colours are formed.

2. The solar spectrum, whether formed by prisms of transparent bodies, or by grooves in metallic and transparent surfaces, consists of *three* spectra of *equal length, beginning and terminating at the same points*, viz. a *red* spectrum, a *yellow* spectrum, and a *blue* spectrum.

3. All the colours in the solar spectrum are *compound* colours, each of them consisting of *red, yellow,* and *blue* light in different proportions.

4. A certain quantity of *white light*, incapable of being decomposed by the prism, in consequence of all its component rays having the same refrangibility, exists at every point of the spectrum, and may, at some points, be exhibited in an insulated state.

This remarkable structure of the spectrum will be better understood from Plate II., where Figs. 1, 2, and 3, represent the three separate spectra, which is shown in their combined state in Fig. 4.

In all these figures the point M corresponds with the *red*, or *least refrangible* extremity of the spectrum, and N with the *violet*, or *most refrangible* extremity; and the ordinates *ax, bx, cx*, of the different curves MRN, MYN, MBN, represent the intensity of the *red, yellow,* and *blue* ray at any point *x* of the spectrum.

If the distance *Mx* in all these spectra be equal, then, in

the combination of them shown in Fig. 4, the ordinates ax , bx , cx , will indicate the nature and intensity of the colour at any point x of the red spectrum. Thus, let

The ordinate for <i>red</i> light	$ax = 30$
<i>yellow</i>	$bx = 16$
<i>blue</i>	$cx = 2$

$$ax + bx + cx = 48 \text{ rays,}$$

then the point x will be illuminated with 48 rays of light, viz. 30 of red, 16 of yellow, and 2 of blue light.

Now, as there must be certain quantities of red and yellow light, which will form white, when combined with 2 blue rays, let us assume these, and suppose that white light, whose intensity is 10, will be formed by 3 red, 5 yellow, and 2 blue rays; hence it follows that the point x is illuminated by

Red rays,	27
Yellow rays,	11
White light,	10

48 rays,

or what is the same thing, the light at x will be *orange*, rendered brighter by a mixture of white light. The *two* blue rays, therefore, which enter into the composition of the light at x , will not communicate any blue tinge to the prevailing colour.

If the point x is taken nearer M, and if, at that point, the *blue* rays are more numerous in proportion to the *yellow* than 2 to 5, that is, if they are as 3 to 5, then there will be 1 blue ray more than what is necessary to make white light with the 2 yellow and the 3 red rays, and this blue ray will give a blue tinge to that part of the spectrum, or will modify the peculiar colour of pure red light. In like manner, the blue extremity of the spectrum may have its peculiar colour modified by an excess of *red* rays so as to convert it into *violet* light. In this manner the tinge of red light at the blue extremity of the spectrum, and of blue light at the red extremity, may be explained, even if the least refrangible branch BM of the blue

curve is every where within the least refrangible branch YM of the yellow curve, and the most refrangible branch RN of the red curve every where within the most refrangible branch YN of the yellow curve. On this supposition the excess of blue light over the yellow will begin to modify the red space at that point where the ordinates cx , bx , are in the ratio of 2 to 5; the ratio in which they exist in white light, and the excess of red light over the yellow, will begin to modify the blue space at that point where the ordinates of the most refrangible red, and most refrangible yellow, branch are as 3 to 5, the ratio in which the corresponding rays exist in white light. But it is not improbable that the blue branch BM may actually cross the yellow branch YM at some point m , as shown in Fig. 5; and the red branch RN the yellow branch YN, so that the blue ordinates in the one case, and the red ordinates in the other, will exceed the yellow ordinates at every point beyond the points of intersection m and n . If this should prove to be true, it follows, that at and beyond n the red should, as it were, re-appear, and by its predominance convert the extreme *blue* space between n and N into *violet*.

In every part of a spectrum thus composed, there necessarily exists three different colours, which form a compound tint by their union; and as the three differently coloured rays have at every point the same refrangibility, it is impossible to separate them, or to analyze the compound tint by prismatic refraction. By transmitting the compound tint, however, through transparent solids or fluids, which absorb one or more of the simple rays, and allowing the rest to pass, we may exhibit one or more of the rays separately, or obtain a residual colour, which indicates the presence of rays whose existence cannot be inferred from the original colour of the compound tint. If, for example, we transmit the compound ray at x , Fig. 4, through an absorbing medium, which detains 27 red rays, we shall obtain a transmitted tint with 11 yellow rays, and 10 of white light, or a brilliant yellow; and if we again transmit this light through another medium which absorbs 11 yellow rays, we shall have a pure white light, composed of 3 red, 5 yellow, and 2 blue rays. This white light will exhibit the singular property of homogeneous light; namely, that of

being indecomposable by the prism, and of being pre-eminently adapted for the nicest purposes of vision. The existence of such light has never even been conjectured, and its insulation at any point of the spectrum becomes a proof of the existence, at that point, of red, yellow, and blue rays of equal refrangibility.

Having thus given a general view of the structure which I have found in the spectrum, I shall now proceed to state the experimental evidence from which it has been deduced.

From the simple inspection of the coloured spaces, it is obvious that *red* light exists in the *red*, *orange*, and *violet* divisions of the spectrum; but, according to Fraunhofer's measurements, these three spaces occupy 190 parts when the whole length of the spectrum is 360; hence, red rays are observed in more than one-half of the whole spectrum. If we examine the *blue* and *indigo* spaces through certain yellow fluids, such as *oil of olives*, they acquire a distinct *violet* tint, so that these fluids must have absorbed some rays which had neutralized or masked the red. Red light, therefore, exists in the *blue* and *indigo* spaces; and, as I shall afterwards show that white light, which necessarily includes red, may be insulated both in the green and yellow spaces, it follows that red light exists in all the seven coloured spaces into which the spectrum is divided.

Yellow light is distinctly recognized by the eye in the *orange*, *yellow*, and *green* spaces which occupy 77 parts of the spectrum whose length is 360. When the spectrum is examined with a deep blue glass, the green light is distinctly seen at F, one of Fraunhofer's lines; and as a green transparent wafer of gelatine produces a whitish band beyond F, and in the blue space, it is clear that a certain portion of yellow light must exist there. We have already seen that the action of oil of olives on the blue and indigo spaces absorbs certain rays and leaves a violet tinge. These rays cannot be red, and they are not blue, because blue taken from blue would not leave violet. They must, therefore, be a small portion of yellow rays, which, forming white with the red, and a portion of the blue, had the effect of diluting the predominant blue light. The existence of both yellow and red rays in the blue and indigo spaces, may be inferred from another experiment. When we trans-

mit the spectrum through a certain thickness of a blue solution of the ammonio-sulphate of copper, the blue and indigo spaces appear to be much diluted with white light, that is, the blue appears to be mixed with red and yellow. Now, if this apparently diluted blue light is a pure homogeneous blue, containing neither red nor yellow rays, it would suffer no more diminution in passing through an additional thickness of the ammonio-sulphate, than white light would do in passing through the same thickness of pure crystal or pure water, that is, it would suffer no perceptible change. But, in passing through the copper solution, the blue becomes rapidly deeper and less white, which can arise only from its absorbing the red and yellow rays, which cause its apparent whiteness. In order to apprehend the force of this argument, we must consider, that though a dark red or a dark blue fluid appear opaque, as they are in reference to white light, of which the one absorbs all the rays but the red, and the other all but the blue, yet, in reference to red and blue light, which each of them freely transmits, they may be regarded as perfectly transparent. Nothing is more remarkable to those who first make the experiment, than the imperceptible diminution of intensity which a beam of homogeneous red light experiences in passing through a great thickness of a red fluid, particularly when the original red beam is produced by transmission through the same red fluid. It is owing to this cause that the colour of a wine-glass, full of port wine, is nearly as deep as that of the wine in the thickest part of a wine decanter.

That *yellow* light exists in every part of the red space, may be proved by numerous experiments. By using a prism of port wine of 90° , or by viewing the spectrum through certain thicknesses of balsam of sulphur, balsam of Peru, pitch, or red mica, *yellow* light can be seen directly at the line marked C of Fraunhofer, which is far within the red space; and, by the absorptive action of these four last substances, the whole of the *red* space has a *yellowish* tint, arising from the absorption of *blue* light. The very same effect is produced, but in a more striking manner, by transmitting the light of the red space through certain yellow, orange, and green transparent wafers, all of which absorb some blue light, and leave the whole

of the red space of an *orange* tint, that is, containing *yellow* light. In support of the opinion that there are yellow rays in every part of the red space, I may adduce a casual experiment of Sir W. Herschel's, *Phil. Transactions*, 1800, vol. xc. p. 255, when he had occasion to view the prismatic spectrum reflected from clear turned brass. "The colour of the brass," says he "makes the *red rays appear like orange*, and the orange colour is likewise different from what it ought to be." From these observations it follows, that yellow light may be traced through all the coloured spaces except the violet, where I have not yet been able to find it; but this is not surprising when we consider the great faintness of the violet rays, and the facility with which they are absorbed by media of almost all colours. Even the deep blue ammonio-sulphate absorbs almost the whole of the violet space, and the smalt blue glass nearly one-half of it, so that it is extremely difficult to subject it to the partial action of absorbent media.

It is obvious, even to the eye, that *blue* light exists in the violet, indigo, blue, and green spaces, which occupy 247 parts out of 360, or more than two-thirds of the whole spectrum. When the most refrangible rays are absorbed by certain thicknesses of balsam of sulphur, balsam of Peru, pitch, or red mica, the blue mixed with yellow, and forming green, may be traced very near the line C of Fraunhofer, which is considerably within the red space. That the blue extends over the whole red space is proved by the same arguments which we used for yellow light; for when the red space is made of an orange tinge by the absorptive action of certain yellow, orange, and green media, this change can be effected only by the absorption of blue light.

Having thus proved that *red*, *yellow*, and *blue* light exist in almost every part of the spectrum, I shall proceed to corroborate these views by showing that *white* light may be actually insulated in different parts of it.

When we look at the spectrum through a particular blue glass of a certain thickness, we insulate the *yellow* space, the colour of which is a rich gamboge-yellow. By increasing the thickness of the glass, this compound yellow acquires the pale straw-yellow colour of the yellow monochromatic flame pro-

duced by the combustion of alcohol and water, or of an alcoholic solution of salt. At a still greater thickness of the glass we produce a *greenish-white* band, which, by changing the glass for a different blue, becomes a *reddish-white* band. If we now mix a solution of sulphate of copper, which acts upon the rays on the red side of the yellow space, with diluted *red* ink, which acts on the rays on the *blue* side of the same space, we shall reduce the rays in the yellow space to nearly *white light*, with a slight tinge of *green*, when there is too much *sulphate of copper*, and a slight tinge of *red* when there is too much *red* ink. This insulation of white light may be pretty well effected by some of the smalt blue glasses acting alone; and in some cases the purity of the light may be increased by a solution of *sulphate of copper and iron*, or even by a *green glass*. The white light thus exhibited may be rendered *yellow* by means of a *yellow* transparent wafer, which absorbs some of its *blue* rays, and *green* by a *green* transparent wafer, which absorbs some of its *red* rays.

From these experiments it follows, that *white light*, composed of *red*, *yellow*, and *blue* rays, exist in the most luminous part of the spectrum, and may be insulated by absorbing the excess of yellow light, and of any of the other colours above what is necessary to compose white light. In applying a highly dispersing prism, it was a singular and peculiarly interesting sight to witness, for the first time, a beam of white light, consisting of *red*, *yellow*, and *blue* rays of equal refrangibility, and incapable of being analyzed by prismatic refraction.

The preceding observations contain only a few out of a great number of experiments which I have made on the absorptive action of natural and artificial crystals, and of various fluids and uncrystallized solids which possess either a natural or an artificial colour. I made a few experiments in the course of this winter with the coloured juices of several hot-house plants, which Mr Forbes was so good as to prepare for me; and I expected, in the course of the summer, to obtain by this means, a more striking insulation of some of the simple colours, than I had effected by the substances within my reach. Impatient, however, of so long a delay, I thought of supplying, to a certain degree, the place of these absorbing fluids by a subsidiary principle of analysis, which, in its practical applica-

tions, exceeded my most sanguine hopes, and has furnished me with the means, not only of analyzing the colours of natural bodies, but of determining the causes from which these colours originate. An account of these, and of other applications of it, will form the subject of separate communications, and I shall confine myself at present to the single observation, that, in applying this method of absorption to the decomposition of the solar rays, I have been able to insulate white light, both in the *orange* and in the *green* space, and thus obtain the most ample proof of the peculiar analysis of white light which it has been the object of this paper to establish.

By means of this analysis we are now able to explain the phenomena observed by those who are insensible to particular colours. (See this *Journal*, No. xix. Old Series, p. 153, and No. ix. New Series, p. 88.) The eyes of such persons are blind to *red* light; and when we abstract all the *red* rays from a spectrum constituted as above described, there will be left two colours, *blue* and *yellow*, the only colours which are recognized by those who have this defect of vision. To such eyes light is always seen in the *red* space, but this arises from the eye being sensible to the *yellow* and *blue* rays which are mixed with the *red* light.

Hence blue light will be seen in the place of the *violet*, and a greenish-*yellow* will appear in the *orange* and *red* spaces, or, which is the same thing, the spectrum will consist only of the *yellow* and the *blue* spectra shown in Fig. 2 and 3. The physiological fact, and the optical principle, are therefore in perfect accordance; and while the latter gives a precise explanation of the former, the former yields to the latter a new and an unexpected support.

ART. III.—*Further Remarks on the alleged Polarization of Heat.* By the Rev. BADEN POWELL, M. A. F. R. S. Savilian Professor of Geometry in the University of Oxford. In a Letter to Dr BREWSTER.

DEAR SIR,

IN the number of your *Journal* for July 1830, you did me the favour to insert a communication on the alleged polarization

of heat, in which I stated, that, as far as I was aware at that time, M. Berard's researches on the subject had not been published. I have, however, recently been informed that I was mistaken; and being referred to the 3d volume of the *Mémoires de la Société d'Arcueil*, (Paris, 1817,) I there found the paper in question, entitled "Mémoire sur les propriétés des différentes espèces de rayons qu'on peut séparer au moyen du prisme de la lumière solaire, par M. J. E. Berard. Lu à l'institut le 21 de Dec. 1812."

I immediately examined the memoir with reference to the question discussed in my former communication; but, I must confess, felt not a little disappointed at the very slight information which the distinguished writer affords as to the particular points on which, it appears to me, so much of the force and importance of the general result depends.

The principal particulars which I can collect as bearing on my questions are these:

1st, The thermometer employed was an *air-thermometer*, containing a bubble of alcohol in the tube.

2d, The apparatus, consisting of glasses like Malus's, had the axis of revolution placed *vertically*. No mention is made of any precautions in regard to the very important point of guarding against irregular action of heat by screens, &c.

3d, With regard to the radiation from hot metal, the source was "une boule de cuivre de la grosseur d'un œuf," which was placed in the focus of a concave metallic reflector. It was used in the first instance at a bright red heat, and in this case, as well as in that of the solar rays, the results agree entirely with what I have before stated.

4th, The author then proceeds to the case of *simple heat unaccompanied by light*: this he seems to consider but of secondary importance, regarding it in connection with the views inferred from De la Roche's experiments, but, after a few remarks, proceeds thus to describe his experiments:

"J'ai attendu dans quelques expériences que la boule de cuivre ne fut plus visible dans l'obscurité; et je me suis assuré que les rayons qu'elle émettait dans ce cas pouvaient être aussi polarisés."

This meagre, not to say somewhat ambiguous statement, is

the whole account given of a portion of the research at once (according to my view of the matter) the most essentially important, and certainly the most difficult and delicate to carry on.

Of the nature of the results in general, so long as any *light* is present, I have already expressed my opinion, and in these cases the causes of fallacy do not act in the same proportion; but in the case of the *simple radiant heat*, I think it will be allowed that a *vertical* arrangement of the apparatus must leave the results open to very serious objections, and the adoption of the *air-thermometer* will by some be thought still more questionable.

In my former communication, I mentioned that my experiments, made with a mercurial thermometer, though unsatisfactory from the minute effects produced, appeared to confirm M. Berard's as to all cases of *heat accompanying light*, but that they failed to do so in the case of *simple radiant heat*. With the information now before me relative to M. Berard's researches, I resolved again to repeat the experiment in the case of simple heat from metal below visible redness, making use of an air-thermometer as he had done. I arranged my apparatus as before, horizontally, the iron ball, two inches diameter, was placed in the focus of a tin reflector, six inches diameter, so situated that the rays would fall on the first glass at the proper angle. The blackened bulb of an air-thermometer was fixed in the focus of the small reflector attached to the second glass; the adjustment being made by the light of a candle. The bulb of the thermometer was three-fourths of an inch in diameter, and the air was confined by a bubble of coloured liquid in the tube, as in M. Berard's experiments. The tube of the apparatus passed through a thick screen, so that all communication of heat to the thermometer, except along the tube, was completely intercepted. Every precaution being duly taken, and the second glass reflector, with the thermometer, being in *azimuth* 90° , on admitting the radiant heat, *a small effect was produced*, and the reflector being then turned into *azimuth* 0° , and back again to 90° several times, *no sensible difference could be observed between the effects in the respective positions*.

Such are the results I have been able to obtain, entirely confirming my former conclusions. The inquiry is certainly one of a very troublesome nature, and where it is difficult to feel perfectly satisfied. The principal obstacle is the want of a thermometer at once sufficiently sensible and accurate. Those who are practically conversant with air-thermometers, will probably be ready to admit that there is much uncertainty attending their indications. It is to the improvement of *mercurial* thermometers that we must look for the extension of our knowledge on this and all questions connected with radiant heat. To secure at once an instantaneous communication of the effect, a large range on the scale corresponding to a small degree of heat, and a perfect freedom of motion to the liquid in the tube, are the desiderata in instruments for these purposes, whilst the graduation is wholly irrelevant. It is to be hoped that some of our philosophical artists will direct their attention to this point.

I cannot conclude without repeating my earnest hope that these experiments will be tried by others better qualified to do justice to them. By the multiplication of observations, we can alone hope to be able to speak with confidence as to investigations of so delicate a nature.—I remain, my Dear Sir, very truly yours,

B. POWELL,

Oxford, July 24, 1831.

ART. IV.—*On the General Equations for determining the Positions and Curvatures of Arches of Aurora Boreales, from observations made in one place only; with Calculations for that of the 25th December 1830.* By R. POTTER Junior, Esq. Communicated by the Author.

SINCE writing my former essay on this subject, I have taken some little pains to learn what has been done by others. In the beginning of the eighteenth century, the meteor appears to have occurred very frequently, and attracted the attention of many scientific men. Amongst these, the celebrated Swedish philosopher Celsius, commencing about the year 1716,

made observations on 316 auroræ, and wrote a treatise wherein these observations are recorded; but which I have not yet met with.

The celebrated Mayer wrote also in the year 1726, pretty fully on the aurora in the first volume of the *Commentarii Petropolitane*; and I was not a little surprised to find that he had proposed a formula for finding the elements of an arch from one observation of its altitude and its extent on the horizon, by considering it as a circle parallel to the equator. The reader will see that this is similar to the method I have proposed, only that he was ignorant of the relation of the meteor with the earth's magnetism. When his formula is adapted to this discovery, we may find the *distance* of an arch by it in the first place, and knowing this, we easily find the height. The results, as I have proved, are just the same as by mine. He there gives only the formula without the demonstration; this last was, however, supplied by Maupertuis in the *Histoire de l'Academie Royale des Sciences*, for 1732. Mayer laid his demonstration before the Petersburg Academy in 1728, but, as their Transactions were not published until some years afterwards, Maupertuis does not appear to have been aware of it.

In the year 1731 Mairan, who had previously written several memoirs, published an extensive treatise on the aurora, which stands as a supplementary part to the *Hist. de l'Acad.* for that year. He was much occupied with hypothetical views, that the aurora was caused by the zodiacal light, and that this latter was no other than the sun's atmosphere extending beyond the orbit of the earth. He gives an extensive catalogue of auroræ which had been observed from very early times, and several very good plates of the different appearance of the meteor. He enters also very fully on the subject of its height, and made calculations for several observations on the parallactic or trigonometrical method; and in one case he made use of the formula of Mayer. He found the height of the aurora of the 19th October 1726, to be 70 French leagues above the surface of the earth; but in general his calculations brought out the height about 200 leagues, some few being as low as 100, and some as high as 300 leagues.

Soon afterwards the celebrated Dutch philosopher Muschenbroek treated very fully, in his physical essays, on the aurora. Muschenbroek made many observations, and was, as far as I have found, the originator of the hypothesis, that the meteor arises from electrical clouds, and is only at a low elevation. Like his successors in the same view, he preferred trusting to the capability of his eyes for ascertaining distances to actual *positive* measurement. As he says every thing, perhaps, which can be said in favour of his hypothesis, I will here give an extract of the summary of his arguments from the French translation of his work by Pierre Massuet, published in 1739.

“ L'Aurore boréale est dans notre atmosphère, et non pas dehors ou au-dessus.

“ 1°. Parceque qu'elle paroît le soir sous la forme d'un nuage, qui ne diffère pas des autres nuages, que nous voions communément. Le grand nombre d'observations, que j'ai faites pendant longtems pour découvrir la nature de cette lumière, m'ont appris, que ce n'est autre chose qu'un nuage semblable aux autres qui roulent dans l'air, où ils sont suspendus, et qu'elle se trouve aussi à la même hauteur que ces nuages, autant qu'on peut en juger par la vue. Si on est curieux de savoir, quelles sont les marques auxquelles on peut connaître cette lumière pendant le jour, il suffit de faire attention à ce qui suit. Lorsqu'il doit tonner, on voit l'air convert de nuées, qui sont à diverses hauteurs, et peu distant les unes des autres : on les voit flotter çà et là dans l'air, et quelques unes de leurs parties parviennent blanches, les autres brunes. Les nuées qui forment l'aurore boréale, sont suspendus de la même manière dans l'atmosphère, roulant confusément les unes sur les autres, de sorte qu'une personne peu accoutumée à observer ces phénomènes jugeroit d'abord que la tonnerre est prêt à tomber sur sa tête. Mais les aurores boréales sont moins épaisses, d'un bleu tirant sur le cendré, elles flottent doucement dans l'air, et ne s'élève au-dessus de l'horizon que jusqu'à une certaine hauteur. Lorsque tout cela se manifeste au nord, au nord-est, ou au nord-ouest, il paroît sûrement la nuit une aurore boréale ; et si cela continue quelques jours et quelques nuits de suite, on a alors pendant le jour une occa-

sion fort favorable pour faire connaître ce météore, &c. &c.— Je crois donc qu'on ne peut douter, que ces aurores boréales ne soient des nuées repandues dans notre atmosphère."

" 2°. Comme la nuée lumineuse se tient plusieurs heures de suites à la même hauteur au-dessus de l'horizon, elle doit nécessairement se mouvoir en même tems que notre atmosphère; car puisque la terre tourne chaque jour autour son axe, cette nuée lumineuse devroit paroître s'élever au-dessus de l'horizon et descendre au-dessous, si elle se trouvoit au-dessus de l'atmosphère. Cette nuée étant donc emportée en même tems que notre globe et notre atmosphère, il y a tout lieu de croire qu'elle s'y trouve effectivement."

" 3°. Il y a plusieurs aurores boréales, qu'on ne sauroit voir en mêmes tems de deux endroits peu éloignés l'un de l'autre, ce qui prouve qu'elles ne sont pas toujours à une hauteur considerable, et qu'elles se trouve sûrement dans notre atmosphère. En effet, on voit cette lumière à Leyden tandis qu'on ne la voit pas à Utrecht, on l'apperçoit à Utrecht lorsqu'elle n'est pas visible à Leyden. Combien de fois ne l'observe-t-on pas en Hollande, quoiqu'elle paroisse rarement en France et encore plus rarement en Italie? Disons donc que c'est une lumière contenue dans notre atmosphère, et qu'elle ne s'y élève pas même fort haut," &c. &c.

His argument against the calculations on the method of finding the height by the parallax is, that different auroræ are viewed by the different observers; though he acknowledges that they are generally seen in the north; which, it would seem, should be considered a very singular thing if they were only in the region of the clouds.

Without entering into the question of the aurora being in the earth's atmosphere, to which we can yet hardly fix a definite limit with certainty, we see that his arguments on its locality and connection with clouds are quite specious and unworthy of a philosopher. What he says as to its frequently not being seen in places only a few miles distant at the same time, amounts, even if it were shown that the observers were on the look-out at the same time, with no obstacle to intercept the view, to a vague negative argument, which would

not render actual measurements the less necessary. And this would become so easy for a low elevation of the meteor.

Those who do not wish to take even so much trouble, may satisfy themselves by viewing a stratus cloud which passes any where near the zenith. If this cloud be really of pretty nearly an equal breadth throughout its whole extent, it will, in conformity with the rules of perspective, *appear* much broader near the zenith than in its more distant parts near the horizon. Now, in a symmetrical regular arch of an aurora, no similar difference in breadth is ever perceptible,—a sufficient evidence of their great distance from the observer.

As to his second argument, it would at most show the meteor to be connected with the terrestrial system, and the view first brought forward by Dr Halley, which has since been completely established by Mr Dalton, that it is connected with the earth's magnetism, is now universally admitted. Nevertheless, the apparent motion of the meteor, and particularly its tendency to pass southward, are still subjects for inquiry and discussion.

It is one of the most singular features in the history of the observations on the aurora, that its connection with the earth's magnetism should have escaped the notice of such inquirers as Mayer, Mairan, and Muschenbroek, who all refer the meteor to the true north, though the magnetic declination was about $15\frac{1}{2}$ degrees west at London in 1731. And though some observations are recorded where it was noticed not to coincide with the true north, it is not the less to the credit of Dr Halley, that this great man, on the first aurora he saw, namely, the famous one in March 1716, which drew the attention of so many learned men throughout all Europe to the subject, perceived at once its great distance and height, and argued also on its connection with magnetic phenomena.

Near the close of my former paper, I remarked that the manner of finding the elements of an auroral arch from an observation of the points in which it cuts the horizon, and the altitude of its highest point, was not the only way in which we might consider the subject; assuming it still to be a small circle round the magnetic axis, and in a plane perpendicular to it; but that we might substitute in place of the equation

of the plane of the horizon, that of any other plane passing through the place of observation.

The most general way of treating the problem is to take the plane at right angles to the magnetic meridian, but making any angle with the vertical line. Thus we arrive at a general formula, which will be discussed below, and from which we deduce that for the plane of the horizon as a particular case, and it is the simplest one.

But it will not be without service to make observations for planes of different elevations on the same arch. For if, as some philosophers maintain, there are more *virtual* magnetic poles than two on the earth's surface, we ought to find considerable deviation from a circle in the direction of the symmetrical rings or arches. But if we find a close coincidence, between the results furnished by the different points on a high and extensive arch, we must consider that there is only one such pole for each hemisphere; and if the electrical theory shall be established, which, from recent experiments, there is reason to expect, we must allow the irregularity in the direction of the magnetic tendency on the earth's *surface*, to arise from partial and local causes, such as the situation of places with respect to large continents and the diurnal rotation, or the effects of prevailing winds, &c.

To proceed to the general problem, let Fig. 6, Plate II. represent a view of an auroral arch, the plane AEX being in the magnetic meridian, and the line AX being its intersection with the horizon. The data required for the solution of the problem, are the angle EAX , or the elevation of the arch, the angle $DA X$, or the inclination of the plane DAF , with that of the horizon, and the angle DAF .

Then if a be the place of observation in Fig. 7, $aM\alpha$ a portion of the earth's surface, MO the magnetic axis, and ef a portion of an auroral arch, we may take the centre O as the origin of the co-ordinates, and put e = the trig. tang. of the angle $e a X$, f = the trig. tang. of the angle $d a f$, i being = the trig. secant, and j = trig. tang. of the angle of inclination $d a X$, and g = the trig. cotangent of the magnetic polar distance of the place of observation, or = the trig. tang. of the angle $MO\alpha$.

Then it will be seen that the arch being at an equal height in every part above the surface of the earth, it will be everywhere equally distant from the centre, * we will represent this distance Oe by R , putting r for the mean radius of the earth, and r' that of the small circle considered in its own plane. We have then for the points of the small circle in the plane of xz , $z = r + ex$, $x^2 + z^2 = R^2$, and $(x - a)^2 + (z - c)^2 = r'^2$, and in the plane daf , $z' = r + jx'$, $y' = f^i x'$, $x'^2 + y'^2 + z'^2 = R^2$, $(x' - a)^2 + y'^2 + (z' - c)^2 = r'^2$, we have also $c = ag$ and $R^2 = r'^2 + a^2 + c^2$.

These equations are sufficient to determine the circle. In the elimination it is better for simplifying the formula to put $l = 1 + i^2 f^2 + j^2$, $m = 1 + e^2$, $n = 1 + g^2$, $o = 1 + jg$, and $p = 1 + e + g$.

We then arrive at this equation,

$$a = r \left(\frac{lp^2g + 2eop - mo^2g - 2jop^2}{lp^2n - mo^2n} \right) \text{ which we see is}$$

of the same form as the one we found in considering only the points of intersection on the horizon; and, in fact, if we make the angle of inclination equal to zero, it then coincides with the horizon; and we have $i = \text{radius} = 1$, and $j = o$, these give us $l = 1 + f^2$ and $o = 1$, and the equation

$$\text{above becomes } a = r \left(\frac{lp^2g + 2ep - mg}{lp^2n - mn} \right) \text{ the same as we}$$

found before. As the height of the meteor, and its distance from the observer, are generally the only elements required to be known, we may shorten the calculation by finding the immediate analytic expressions for them. Thus, the height being $R - r$, we have only to find the value of R and subtract from it the radius of the earth; but substituting the value of a in the above equations, we find for the horizontal plane the follow-

$$\text{ing easy and short formula, viz. } R = 2r \sqrt{\left(\frac{e^2lp^2}{(p^2l - m)^2} + \frac{1}{4} \right)}$$

* We here neglect the spheroidal figure of the earth, as it remains yet to be determined whether the observations can be taken so accurately, on such a meteor as the aurora, as to render such nicety in calculation of service. If it is found that they can be, it will become necessary also to correct the angles observed, from the consideration, that the arch is a view of a cylindrical ring, of which the figure of the section would require to be ascertained.

Then knowing R , r and the angle $e a O$ in the triangle $e a O$, we find the distance, or the side $a c$, by the ordinary trigonometrical rule.

When we take the extent of the arch, on any other plane than that of the horizon, the formula for the value of R becomes less simple; by following the same plan as before, we find this formula, viz.

$$R = 2 r \sqrt{\left(\frac{e^2 l o^2 p^2 + j^2 m o^2 p^2 - e j m o^5 p - e j l o p^3}{(l p^2 - m o^2)^2} + \frac{1}{4} \right)}$$

And when we know R , we easily find the distance as in the other case.

I have not yet been able to make observations for any other plane than that of the horizon, but if the aurora shall occur as frequently next winter as it did the last, I hope it will not pass over without some observations being obtained which will tend finally to settle the question of the height of the meteor above the surface of the earth. I propose keeping a vigilant look out for displays of it on which I may myself make observations with such an instrument as I have. But I should be glad to hear that others, with better instruments, may feel sufficient interest in the inquiry to seek opportunities of observing. For the guidance of such friends of science, I will take the liberty to suggest, that it will generally be found best to take the measurements for the under or inner edge of the arch, when it is not a very high one, above the horizon; for in this case the under edge is almost always well defined, whilst its upper one is frequently so diffuse that two observers would seldom agree to less than 30' of a degree where it ended. When the arch passes at an altitude approaching the zenith, it will generally be immaterial on which edge the observation is made.

I have not yet met with an observation which would enable me to calculate the arch of the 25th December last, by the parallaxic method, but I hope some observer will yet appear who has recorded an observation of it sufficient for the purpose. In the meantime, I give the height and distance as brought out by the analytical formulæ.

The lower arch of the aurora had an altitude of 20° for its under edge in the magnetic north, and it cut the horizon about 61° west of this point for the same edge.

Therefore, taking $e = \text{trig. tang. } 20^\circ$, $f = \text{trig. tang. } 61^\circ$, and the magnetic polar distance of the place of observation being 41° , we have $g = \text{trig. tang. } 49^\circ$. From these data I find its distance to have been 412.5 miles, its height 159.4 miles.

In looking over the history of the aurora, we see that the calculations of Mairan, Cavendish, and Dalton, on various trigonometrical bases, of from about thirteen miles to some hundreds, agree with mine in placing the meteor always at a great elevation, without a single opposing measurement from good observations in favour of the contrary opinion. The lowest altitude determined, namely, that for the aurora observed by Cavendish, had still a locality such that it is clear it must be ascribed to other causes than the winds and clouds of the denser part of the atmosphere.

Though, from its position, we are hardly ever likely to be able to demonstrate actually in what these causes consist, we may yet, by comparing the different theories which can be advanced, form our choice of the one which is most consonant with our previous knowledge and the laws of nature hitherto established. And as my own opinion, founded on observation and comparison with other natural phenomena, is somewhat different from any I have seen, I shall deem no apology necessary in here submitting it.

I have been struck with the similarity of the light generally called electrical in the *vacuum* of a mercurial barometer and that of the aurora. The exhibition of this light has generally been considered a test of the goodness of the instrument. But it may be seen very completely in the comparative barometer attached to the condensing steam-engine, which generally in a large long-stroke engine, oscillates between 1. and $1\frac{1}{2}$ inches every stroke of the piston, and about two to three inches of mercury below the atmospheric pressure. When the mercury is falling in the tube there is seen a pale flickering light, which ceases when the mercury commences to rise again. That is, the light is seen only when the vacuum (*as used in common parlance*) is becoming worse and the mercury falling.

In whatever way we may explain this phenomenon, the light of the aurora has a very great resemblance to it.

But if we consider this light as arising from the mercurial vapour, it will be asked, from whence could such a metallic vapour arrive at the highest regions of the atmosphere? In answer, I would say, tell me whence come the meteoric stones, and I can give as likely an origin for such a metallic vapour. He would be very hardy at the present day, and I think little versed in chemistry, who would venture to assert the non-existence of metals in such a state, even at low temperatures. We know that aqueous vapours exist at temperatures below the freezing point, and we see that many metals, as zinc, arsenic, mercury, &c. are easily distilled. We find also, by the evidence of our sense of smelling, though so much inferior in the human species to what it is in many of the lower animals, an impression of the existence of a metallic vapour, when we are in places where metals are worked. But for a ready experiment, let any one take a piece of iron or steel, and rub away the coating of varnish or oxide which may be on its surface upon a rough stone or file, on removing it to a short distance, he will readily perceive, if his olfactory sense be not particularly dull, a peculiar metallic odour, which cannot arise otherwise than from the iron, and which is only perceived in its vicinity.

I must acknowledge that there is *quite* as much difficulty in accounting for metals in this state floating in space, as there is in accounting for the origin of aerolites. But the phenomena of the aurora are undoubtedly, in my opinion, best accounted for, on the view that the metals which form so principal and characteristic a portion of meteoric stones exist also in a state of vapour, moving in space; which, arriving within the influence of the earth's attraction, are brought into a state of commixture with the higher portions of the atmosphere.

The forms of the meteor, the peculiar light, and the positions with respect to the magnetic tendency, must then result from the necessary effect of the earth's electrical or magnetic state.

The rarity of the matter which causes the aurora is quite in agreement with the tenuity which must exist in a metallic vapour at exceedingly low temperatures. For we find a mass of forty to fifty miles in depth, hardly, in a telescope, affect

the intensity of the light of very small stars. The density of these masses, as I have computed, must be *less* than the one ten-millionth part of that of the mercurial atmosphere in the Torricellian *vacuum*, to give such a light as we observe,—a tenuity we can scarcely form a conception of.

SMEDLEY HALL, *August 3, 1831.*

ART. V.—*Contributions to Scientific Bibliography.* By a CORRESPONDENT. Communicated by the Author.

THE scarcity of a book, which has nothing else to recommend it, is a fact which is hardly worth knowing ; but when the work has any real value, either from intrinsic merit, or from forming a link in the history of science, its scarceness then becomes an object of importance. It is constantly found that errors are perpetuated by writers who satisfy themselves by copying what they find at second hand, and these errors can never be corrected without reference to original authorities. It is useful, therefore, to know not only what books are scarce, but also (when it is possible) where they are to be found. It must often occur to those who have access to large libraries, that they meet with works which they had never before heard of ; and the books so discovered will sometimes contain matter which had been looked for in vain. Neither is it to be attributed to a mere selfish anxiety for accumulation, when any one employs himself in collecting with this view. Much is learned from what is in our own possession, and even in the search for it ; and every one who has indulged himself in collecting books, must have often found advantage from what, at the time of the purchase, he did not foresee that he should derive any particular use. This of course supposes that he has collected with some discrimination ; and if in that case he should sometimes be induced to give a higher price than he otherwise would for a book in consequence of its scarcity, he is not on that account alone to be blamed. There are other individuals also to whom some anecdotes on this subject may not be uninteresting ; and the following remarks may hereafter be suc-

ceeded by others of a similar description, if they are found to contribute even to liberal amusement.

Briggs's Arithmetica Logarithmica, 1624.

Dr Hutton gives an account of "this stupendous work," as he justly calls it, in his well-known history of logarithms. He furnishes an ample detail of its contents; but in some of the particulars respecting the book itself, his description appears to require correction. The book contains, as he observes, the logarithms of all numbers, from 1 to 20000, and from 90000 to 100000 carried to fourteen places of decimals; but he adds, "some writers say that there was another chiliad, namely, from 100000 to 101000; but none of the copies that I have seen have more than the 30000 above-mentioned, and they were all regularly terminated in the usual way with the word *Finis*." Gardiner, in the preface to his quarto *Tables of Logarithms*, (London, 1742,) mentions his having particularly used this chiliad, but it certainly is hardly ever to be found. There are, however, at least two copies of it in existence; the one is in the Savilian Library at Oxford, and the other is in the possession of the Earl of Macclesfield. It is evident that it formed no part of Briggs's original design, but was printed separately after the great work. It consists of only a few leaves, and these circumstances will account for its having been so generally lost.

From the letters with which the different sheets are marked, it is clear that Briggs's original intention was not to leave his great work in the imperfect state in which it has come down to us; and indeed Wingate says in his preface to *Tabulæ* Logarithmicæ* (Lond. 1633,) that after the publication in 1624, he "endeavoured, with all the expedition that he and his friends could make, to publish a perfect table, which should contain the logarithms of all numbers from 1 to 100000. Howbeit, before it could be quite finished, that table which he intended was brought home to him ready calculated and published in the low countries without his consent or notice, he having directed an easy way (in his book above-mentioned)

* These are only for 7, not 8 place of decimals, as Hutton describes them.

how the logarithms of the intercepted numbers might be discovered."

The copy of the *Arithmetica Logarithmica* in the Savilian Library was probably Briggs's own, and there is between the leaves of it a letter which he had written to his friend Mr Wells of Deptford, giving instructions how to proceed in calculating for him. This agrees well with Wingate's statement; and it is to be regretted that Vlacq should have interfered with the author's plan. If he had not, we should have had the work completed for the whole series of numbers to fourteen places of decimals. Vlacq's, indeed, go to ten places; but his hurry to anticipate those who ought not to have been interrupted, made him suffer his work to go through the press in a most careless manner. The number of errata is really disgraceful, and particularly to be reprobated in a publication like this. It is true that figures require more than common care in the revision of the sheets, but it is equally true that errors in printing them are of more than common importance.

Besides Vlacq there was one of his own countrymen whom Briggs had likewise some reason to complain of; but Hutton's statement leaves an inaccurate impression of what he did. He says, "In 1631 was printed in London, by one George Miller, a book containing Briggs's logarithms, with their differences, to ten places of figures, besides the index, for all numbers to 100000, as also the logarithmic sines, tangents, and secants, for every minute of the quadrant, with the explanation and uses in English." He then gives a quotation from Norwood's *Trigonometry*, in which this publication as well as Vlacq's are severely reprobated; but the fact seems to be much over-stated. Miller published a title-page with an imperfect preface made up from Briggs's Latin, but it does not appear that he ever reprinted the logarithms. This English preface is in the British Museum bound up by itself. It is sometimes met with in the same volume with Briggs's own logarithms to fourteen places, to which it certainly cannot belong; but a copy in the Savilian Library clearly points out the distinct object with which it was printed. It is there prefixed to Vlacq's own book, to which the description in the title-page exactly answers. To remove all doubt on this subject, it is only necessary to com-

pare the tables of errata, and it will be found that the English is a mere translation of the Latin table given by Vlacq. This clearly proves that it was never intended for any reprint of the work which Miller might have had executed in England, but only to assist the mere English reader in the use of the Dutch publication. This certainly was an assistance to the circulation of what might be considered in some measure as a pirated book, but does not amount to a substantive act of piracy in itself.

ART. VI.—*Silver Ores reduced by the Method of Becquerel.*

By ANDRES DEL RIO. (Read November 5, 1830.) Communicated by a Correspondent.

I HAVE the honour to present to the American Philosophical Society, the result of some curious researches, which in more dexterous hands may become interesting. They were suggested to me by the beautiful experiment of M. Becquerel, inserted in the *Annales de Chimie et de Physique* for September 1829. He introduced into a glass tube some carburet of sulphur, with a solution of nitrate of copper, which, being of less specific gravity, floated upon its surface; and by means of a copper wire he established a communication between the two liquids. He observed that the surface of the wire became coated with protoxide of copper, while small tables, assuming a metallic and glistening appearance, were deposited on the sides of the glass tube. These M. Wohler has since shown to be formed of sulphuret of copper; whence he considers the method of Becquerel as being merely “a new mode of forming sulphurets,” to which, I think, he should have added, “by the decomposition of other sulphurets.” Indeed, I introduced severally into three small glass tubes, some small lamellæ of ductile and some fragments of brittle silver glance and red silver ore. These, being exposed to the action of nitrate of copper and a copper wire, were reduced in eight days to the state of metallic silver.

I repeated the experiment on the ores in small fragments, which became coated with silver in five days. The formation of silver was even apparent on the second day at the points in which the ore came in contact with the glass. By what pro-

ness nature invests silver glance and red silver ores with native silver in the mines, is a question which I do not pretend to solve.

In operating upon the fragments of silver glance, which were coated in the preceding experiments with metallic silver, I added a little quicksilver. In fifteen days the ore was transformed into an amalgam of silver. The brittle sulphuret and the red silver ore required nearly three weeks for their complete amalgamation. According to M. Wohler the carburet of sulphur is decomposable in the same way by diluted nitric acid. I did not succeed in decomposing the dark red silver ore by nitric acid, until after a subsequent saturation of the acid with copper.

These experiments afford us instances of the reduction of silver ores without common salt, and sulphuret of iron and copper (the *magistral* of the Mexicans.) I always suspected that the latter substance was very mischievous in the process of amalgamation, occasioning the great loss of silver and mercury which are daily experienced. The silver being oxidized at the expence of the sulphuric acid, retains its oxygen with more tenacity, as has been shown by M. Berthier, than had been previously admitted; and especially when in contact with the oxide of copper, which possesses this property to a high degree. The same probably happens with the mercury; and the sulphates, bisulphates, and subsulphates, which are thus formed, are lost for ever in the process of washing. Hence, I think, they roast their silver ores in Saxony with common salt alone; and they treat the chloride of silver by mercury. By this means the loss of mercury amounts only to four ounces per five mares of silver obtained in the working of ores that contain two ounces per quintal. (See Note A.) How satisfied would we be in Mexico if our loss were no greater?

I trust that the pupils of the College of Mines of Mexico may derive some advantage from these small experiments of mine. Perhaps I have thus approximated to the discovery made by my lamented pupil Valencia, and which he unfortunately carried with him to the grave. I can state as a fact, that by his genius he had discovered the means of avoiding the loss of mercury termed the *consumido*. Such are the fruits

resulting from colleges of mines. In my last letter I suggested to him the use of the protomuriate of tin, not of the nitrate of copper; and in his reply he informed me that his method was so simple, that he feared the workmen would deprive him of the fruits of it. As the mail was closing, he postponed the communication to his next letter, which, however, he never had an opportunity of writing, having previously, as well as many others of my able pupils, fallen a victim in the war of independence.

Should the Philosophical Society continue their liberality to me, by supplying me with a fragment of the white silver glance (*weisguld* of the Germans), I propose to extend my experiments to that ore.

Note A.—The Mexican amalgamator divides his loss of mercury into two parts; the first he terms *consumido*, which is always equal at least to the weight of silver obtained. The second termed *perdida*, is the waste in washing, &c.

ART. VII.—*A Letter from CHARLES CHEVALIER, Optician in Paris, to Dr GORING, being an Answer to his Paper published in the Second Volume of the New Series of the Quarterly Journal of Science, p. 248, entitled, "A Critique on the thick Aplanatic Object-Glasses for divergent rays of Vincent Chevalier ainè et fils."* Communicated by Dr GORING.

SIR,

I HAVE read with the greatest interest the different papers you have published on the microscope, and which have greatly contributed to the improvement of those useful instruments.

As you have had the kindness to honour our labours by writing an article or critique upon the achromatic object-glasses which we worked for Mr Lister, I hope that you will receive favourably the details which I here transmit to you relating to our achromatic microscopes, and which are in answer to your critique, bearing date from October to January 1828, (See Dr Goring's Remark A, p. 238,) in which, after having examined the different object-glasses of Mr Lister, you make several observations, to which I reply.

It is very true that the achromatic glasses composed of a flint-glass, and a crown-glass, or double object-glasses, give rays well corrected only for the central pencils, and confuse the oblique ones ; but their advantages in clearness and in facility of construction, make them generally preferable to triple ones.

If the object-glass of Mr Lister has its concave tarnished, the fault is to be attributed to the flint-glass of Guinand ; and as to the appearance of dust visible in the substance which unites the two glasses, we can assure you that it is almost impossible to avoid that very slight inconvenience in any object-glass, whether cemented or not. The advantage of the glued glasses is, that, if they include a little dust, they do not retain any moisture between the surfaces which are in contact.

The object-glasses of ten and fourteen lines must necessarily be ineffective upon test objects, considering their focal distances.

It is probable that some of our first object-glasses have not been perfectly adjusted ; but this is also an imperfection which it is not possible to avoid, rigorously speaking, in the construction of these minute glasses. For the rest, the approbation you concede to our lenses in these words, “ nothing can surpass the beautiful simplicity of the curves of M. Chevalier,” is doing us a great honour. (See remark AA.)

Euler composed his object-glasses of two bi-convex lenses of crown-glass, and a bi-concave of flint, to form a triple object-glass of six lines focal distance, (see the following extract from the work of Euler,) with a large aperture ; and it is because our microscope is achromatic, (though the lenses are differently disposed,) that it is established on the principles of Euler.

You find our quadruple combination of two double object-glasses perfect ; but now we combine together three lenses of short focal distances to form sextuple object-glasses. This combination is scarcely possible except with our lenses, which have the indispensable requisite for the perfection of microscopes, of being very thin, and of a very small diameter, which conditions cannot be easily fulfilled in triple glasses.

Our object-glasses are glued together with a mixture of tears of mastic and turpentine, and that process ought to appear as good in theory as it is in practice, the surfaces in contact hav-

ing rigorously the same radius, and being joined together by a substance as transparent as glass, which obliterates their surfaces. The introduction of dampness is prevented, and the reflections produced at their interior surfaces are neutralized; therefore they are much more transparent. Being, moreover, invariably fixed, they can be employed with as much facility as simple lenses. This it is that makes me think that this is the true method of adjusting achromatic object-glasses, and that if the practice has not been extended to all, it is because the glueing has not been well performed; if well executed, it is always advantageous.*

It is true that we were too much impressed with the idea, that achromatism being once obtained, distinctness and the condensation of an artificial light ought to be sufficient; but after your learned advice, we could observe that a large aperture was also an improvement (*perfectionnement*,—see Remark B at the end,) which must be obtained in microscopic lenses; and that if the effect of object-glasses does not entirely depend upon their aperture, at least, *ceteris paribus*, the object-glasses which admit the greatest opening are the best and the most *penetrating*.

Since the object-glasses were sent to Mr Lister, one of those systems of six glasses, composed of three object-glasses placed one upon the other, each four lines focal distance, has been sent to you. (See Remark C at the end.) I have studied the letter in which you make some very clever remarks upon that sextuple achromatic system. You say with reason that achromatic lenses must be *achromatic*, if it was merely on account of their *name*. The advantage of the set in question you admit to reside rather in their aplanatism than in their achromatism, because you have perceived that a little colour does not alter their performance sensibly, as you have seen with them the lines and lozenges on the scales of the *Podura plumbea*. (See Remark D.)

I say on this occasion, that it is very difficult to judge of the perfect achromatism of a lens, or of a system of lenses; for

* The cementing of achromatic object-glasses was first executed in France, together with the practice of burnishing them into settings for the purpose of combination with facility.

with the most achromatic glass, by a simple modification of illumination, we can show the outlines of the object fringed, with colours, but by a little change in the position of the reflecting mirror, this can be made to disappear entirely. (See Remark E.) This *iris* or rainbow often proceeds from the eye-glass. This is so true, that, with a catadioptric microscope, it happens sometimes that the outlines of objects are coloured.

I believe that when we compare the effect of two achromatic lenses, they must be of the same focal distance, if you wish to judge rightly of their merit, as to their freedom from dispersion and spherical aberration ; therefore, it would not be right to compare (with reference to these qualities,) a lens of six lines, or of an inch focal distance, with an achromatic system representing a power of less than one line. The best object-glass, in my opinion, is that which is the most *penetrating*, viz. that which shows with most nicety the little details of test-objects, even if it gave a little tinge of colour, which it is almost impossible to destroy without a great reduction of the aperture ; but then what is the use of a perfectly achromatic object-glass, if it has no power over the test-objects ?

Certainly I do not deny the advantage of a glass of short focal distance perfectly free from the aberration of sphericity and refrangibility, as it would fulfil the conditions of the highest degree of perfection. I have seen achromatic lenses of all manufacturers. More than a thousand of these glasses have passed through my hands, and I have never found that ultimatum so much to be desired ; therefore, I still believe that the most penetrating object-glass will be the most useful, and that to which the preference must be given. As to other matters, be pleased to let me know what you think of the system of lenses, belonging to Mr Cuthbert, which is composed of the lenses marked.

- | | | |
|---|-----------------------------|--------------------|
| 0 | of 12 lines focal distance, | lens plano-convex. |
| 1 | of 4 lines | do. - - do. |
| 2 | of 4 lines | do. - - do. |
| 3 | of 2 lines | do. - meniscus. |

Three of these lenses are always employed together, whether you take away the glass marked 3 or that marked 0, and em-

ploy the object-glasses 1, 2, 3, to obtain the maximum. (See Remark F.)

The examination which you will be pleased to make, Sir, of the last sextuple object-glass which I have addressed to you composed of

1 lens meniscus, focal distance 4 lines.

1 do. - - 3

1 do. - - 2

will confirm, I hope, the very correct opinion you have that the *perfection of object-glasses is found in short foci*. (See Remark G.) Therefore, it is towards this point that our exertions will be directed, and I think that in a few months we shall increase the power of our last set of object-glasses, (see Remark H) composed of

1 lens plano-convex, focal distance 4 lines.

1 — meniscus - - 2 do.

1 — do. - - 2 do.

I think that opticians must direct their attention and their care to achromatic lenses, of a large aperture, thin, of small diameter, and of the shortest possible focal distance, and when they have succeeded in perfecting them, then will microscopes be carried to their highest degree of excellence.

I have read the interesting paper of Mr Lister upon a new method of correcting the object-glasses of microscopes. I think that this plan will give facilities for the fabrication of object-glasses of a moderate focus, and that by this discovery, Mr Lister has advanced a step farther towards the perfection of microscopes.

Professor Airy proposes in his papers to supplant the achromatic system invented by Dollond and Euler (which is founded on the marvellous structure of the eye) by another process, in order to ascertain to which the preference should be given. I believe it will be sufficient to consult a work, the title of which is, *Dioptricæ*, auctore Euler, Petropoli, 1769, 1771, and the *Analytische Dioptrick*, Leipzig 1778, or better to compare a good achromatic microscope made according to Euler's system, with the best instrument constructed by that of Mr Airy.

In replying to your critique, I find myself under the necessity of making known the different improvements made in microscopes since their origin, more especially as to what concerns achromatic lenses ; I shall therefore endeavour to trace a short notice concerning them, in which I shall enlarge particularly on what has been done in France in this part of dioptricks. In general, the invention of the compound microscope is attributed to Drebbel in 1621 ; there are also some opinions in favour of Zacharias Jansens or Joannidas de Middlebourg, and Fontana. Hook improved it by the addition of several lenses. Huygens investigated its theory as well as Toricelli, who perfected the optical instruments then known.

Since their time many of the most celebrated geometers and artists have occupied themselves with the improvement of microscopes, and have principally sought to construct an instrument which, endowed with considerable magnifying power, light, and clearness, should also be of easy and universal application.

Schmitz, Cuff, Euler, Dollond, Adams, Delabarre, Amici, and Charles, much improved compound microscopes ; for two centuries they were constructed in England, in Holland, in Italy, and in France, until this part of the science of dioptricks seemed to be exhausted. Nevertheless new experiments demonstrated that these instruments were still far from perfection ; in fact, an achromatic combination had not been employed in the composition of object-glasses, which is nevertheless an indispensable condition for the attainment of perfect images.

Euler first occupied himself in the perfection of this instrument. To him it in fact belongs to have applied to microscopes the happy combination of Dollond. (See remark I.) Although achromatic lenses were invented in 1760, yet it was not till 1774 that Euler proposed to employ them in microscopes, and so slow and difficult is the progress of the arts, that it was not till the end of 1823 that achromatic lenses for the microscopes were actually constructed. Even in 1821, M. Biot remarks in his *Traité de Physique*, “ that a great part of the imperfections of microscopes depends on a want of achromatism, which becomes the more insupportable when we wish to use high powers. It is unfortunately impossible,” he adds,

“to correct this defect entirely, since we cannot achromatise lenses as small as those which the microscope requires.” *

About this epoch the celebrated Bavarian optician, Fraunhofer, constructed achromatic lenses having long foci, and consequently they were inefficacious upon difficult objects; and they could doubtless be applied only for the observation of objects of large size, which is not generally the purpose of a microscope.

At the end of 1823, Vincent and Charles Chevalier, opticians in Paris, began, on the recommendation of M. Selligie, to construct an achromatic microscope, whose object-glass was composed of four achromatic compound lenses of two lenses, each of the compound lenses having a focus of eighteen lines, with a diameter of six lines, and a thickness at the centre of two lines. These object-glasses were placed, the one before the other, in order that they might be employed, together or separately, in the manner long practised with simple lenses. The aperture placed between these lenses was one line. This microscope was presented to the Academy of Sciences of Paris on the 5th April 1824. M. Fresnel made a favourable report upon it on the 30th of August following. As M. Selligie did not inform M. Fresnel that these opticians had taken an active part in the construction of this instrument, and that they had made sacrifices of all kinds for its success, they abandoned M. Selligie and his imperfect microscope, (see Mr Lister's Memoir,) in which, however, he gave the happy idea of combining achromatic object-glasses,—a method generally adopted, and to which is owing its actual superiority.

MM. Chevalier and Son were, from that time, occupied alone in the improvement of the object-glasses and of the microscopes; and in September 1824, they had constructed the

* This natural philosopher does not mention the attempts made by Dellond, Pollard, and several other amateurs, or English and French opticians, nor the four object-glasses with three lenses, which, having belonged to M. Charles, are in the Physical Cabinet of the Conservatory of Arts in Paris. The same may be said of the similar works undertaken in 1815 by M. Amici, because the object-glasses then made gave unsatisfactory results, and were consequently useless, having only a nominal property.—*Note of M. Chevalier.* (See Remark K at the End.)

first plano-convex achromatic with two glasses united, of a short focus, thin, and of a small diameter. (See Remark L.) This lens had a focus of four lines, a diameter of two lines, and a thickness of one line at the centre, and it was with lenses of this kind that the improved microscope was constructed which was presented to the *Société d'Encouragement* on the 30th March 1825. On this occasion the reporter, M. Hachette, remarked : “ The images seen through this microscope are free from every sensible aberration, either of sphericity or refrangibility : objects, whether opaque or transparent, appear in it with a distinctness equal to that of achromatic telescopes.

The first achromatic object-glass which had been made in England was the triple object-glass of nine-tenths of an inch focus, executed in 1824 and 1825 by Mr Tulley, at the desire of Dr Goring, (See Remark M,) and of which an account was published in different scientific journals. Mr Tulley afterwards greatly improved his first work, and Mr Dollond was also occupied with this part of dioptrics. With respect to Dr Goring, he has, by means of his learned memoirs, done a service to the opticians of every country, by pointing out many methods of improving the microscope.

On the 3d October 1826, Professor Amici wrote in the following manner to MM. Chevalier and Sons : “ I have received your letter of the 24th September 1825, as well as the notices and the memoir which you have addressed to me. In thanking you for this present, which has been very agreeable to me, I have learned, with great pleasure, that you have arrived at a perfect construction of achromatic object-glasses for microscopes. This interesting part of optics has been generally neglected, perhaps owing to the great difficulties which it presents, and science still requires that skilful opticians should occupy themselves with the amelioration of the achromatism in lenses of short foci. Naturalists will be happy to learn that you have offered them microscopes according to the principles of Euler, which surpass all dioptric microscopes. I hope it will not be long before I shall have an opportunity of admiring your instrument at Paris, and I shall then have the pleasure of showing you a small work of this kind, which, as an amateur, I have constructed for myself.”

According to Mr Lister, Mr Amici abandoned in 1815 his first attempts upon achromatic glasses, because they were not equal to his reflecting microscopes; and it was not till 1824, after the report of M. Fresnel, that the philosopher of Modena undertook this work, by adopting the system of M. Selligue with regard to the superposition of object-glasses. He pursued this idea with great success, for on the 25th May 1827, he brought to France a microscope whose object-glass was composed of three lenses superposed, each having about six lines of foci, and a large aperture, but not cemented. This instrument, united to a great magnifying power, a remarkable distinctness, as well as the horizontality of the instrument, and excited the admiration of both philosophers and amateurs.

During the stay of M. Amici at Paris, Vincent and Charles Chevalier exhibited in the Louvre a horizontal microscope, (see Remark N,) constructed on the principles and after the advice of M. Amici; and this instrument was deemed worthy of the silver medal by the central jury.

Since that time several object-glasses have been made by Tulley, Amici, and Vincent and Charles Chevalier. These glasses were the subject of different memoirs or commentaries by MM. Amici, Goring, Lister, Lebaillif, which have greatly contributed to bring achromatic microscopes to a high degree of perfection; for we can obtain in France, in Italy, and in England, efficacious lenses of four lines focus. The latest trials made in France have proved that it is possible to make achromatic lenses of two lines focus; and that we may even hope to carry still farther the power of dioptric microscopes, such, for example, as to form a compound object-glass of two lenses of one and a-half line focus, or of three lenses of two and a-half lines focus. We may then say after this, that in a few years microscopes will be brought to the highest degree of perfection, since it is probable that we shall then have made the most powerful achromatic systems.

CHARLES CHEVALIER.

PARIS, QUAI D'HORLOGE, No. 69,
10th February 1831.

ART. VIII.—*On the Perfection of Microscopes.* By LEONARD EULER. Communicated by Dr GORING.

THE best microscopes which have hitherto been made are yet subject to such great defects that we have reason to be surprised that the most skilful artists have not been able to free them from those defects, while they have worked with so much success at the perfection of the telescope. In the first place, we remark generally in microscopes, the great defect, that they represent objects with less distinctness than telescopes, which would be entirely rejected if they exhibited such a great degree of confusion as we are accustomed to suffer in microscopes. And we must admit that the more we increase the amplification of microscopes, the more the confusion is increased, so that we can distinguish almost nothing in them. The two sources of confusion, the one being the aperture of the lens, and the other, the different refraction of the beams, concur equally to render insupportable the confusion with which microscopes represent objects.

The best method of diminishing this confusion, would be, without doubt, in diminishing the aperture of the object-glass; but then we should lose in clearness, which is generally so little, that in great magnifying powers it is almost impossible to distinguish the different parts of the object under examination.

It is also a very great inconvenience in microscopes, especially in simple ones, that we are obliged to approach so near to the objects to put them in the focus of the object-glass; because if there is any inequality in them, it is absolutely impossible to recognize the points which are a little out of the focus.

The surest means of remedying all these defects will be, without doubt, to employ, as in telescopes, compound object-glasses of different kinds of glass; and for that purpose, the last object-glass in which we have introduced the new species of flint-glass which produces a great dispersion of the rays, furnishes us with a method well fitted to carry also the microscope to the highest degree of perfection, as it will be easy for a skilful artist to execute such object-glasses which would

have only half an inch of focal distance, and which could receive an aperture of the eighth-part of an inch in diameter. This would furnish for all the magnifying powers a sufficient degree of distinctness; but as such object-glasses will cause no confusion either from the aperture or the different refractions of the rays, the greatest advantage, without doubt, will be that we could see all objects with the greatest clearness and distinctness, which will enable the natural philosopher to bring microscopic observations to the highest degree of perfection.

To facilitate the more the execution as well as the use of such microscopes, we shall give here the description of one, which contains only two eye-glasses, and which might serve even to produce all the magnifying powers, from the smallest to the greatest, without being obliged to change any thing, either in the object-glass, the eye-glass, or in the place of the object, which should be put always at the distance of half an inch before the object-glass. The only variation is to be produced in the distance between the object-glass and the first eye-glass, which will be removed in proportion as we wish to magnify the object.

Description of the Object-Glass.

1. The object-glass should be composed of three lenses, of which the first and third are made of crown-glass, and the second of that sort of flint-glass whose index of refraction is 1,600; so that the focal distance of the whole object-glass may be half an inch, and that it may receive an aperture of the one-eighth of an inch in diameter. For this purpose the three lenses of which this object-glass is composed, will represent little disks, whose diameter will be about the one-seventh of an inch, and they must be made as thin as their figure will allow. For the figure of each we shall give here the measures expressed in thousands of an inch of twenty-seven millimetres.

2. The *first* of the three lenses, the one which is turned towards the object-glass, must be of *crown-glass*, equally convex on both sides, so that the focal distance will be 0,286, and therefore the radius of each of its sides 0,301.

3. The *second* lens of *flint-glass*, equally concave on both

sides, having its focal distance 0,229, and therefore the radius of each of the two sides 0,274.

4. The *third* lens of *crown-glass* must be equally convex on both sides, so that its focal distance may be 0,375. We must give to the radius of the front side 0,644 of an inch, of the other side 0,287.

5. We must combine these three lenses, so that the distance between the middle of the second lens and that of the first or third, should be only 0,019, so that the thickness of the whole object-glass will be about 0,057.

6. The object which is to be examined must be always placed at the distance of half an inch from the object-glass; and then there will be no reason to fear that the too great proximity of the object may cause the least inconvenience. On the contrary, we shall see all the different parts of the object nearly with the same degree of distinctness.

Description of the two Eye-Glasses.

7. It will be proper to form them both of flint-glass, for the purpose of admitting a greater aperture, which will contribute greatly to increase the apparent field, making both of these glasses equally convex on both sides.

8. The first of these eye-glasses, the one which faces the object-glass, must have the radius of each side 1,200, and the diameter of its aperture 0,600.

9. The second eye-glass, turned towards the eye, must have its focal distance 0,333, and the radius of each side 0,400, and the diameter of the aperture 0,200.

10. Beyond this glass we must place the eye at the distance of 0,167 of an inch.

11. The distance between these two eye-glasses will be always $\frac{2}{3}$ ds of an inch, as, according to the nature of the eye, the last eye-glass must be sometimes nearer and sometimes farther from the first, which is effected very easily by means of a screw.

Having made these three glasses, the magnifying power will depend only on the distance between the object-glass and the first eye-glass, to which it is always proportional. But we must remark, that the more we increase the magnify-

ing power, the less will be the clearness, and the portion of the object which we can see at once.

As to the degree of clearness, it will be proper to observe, that, if the magnifying power is under 20 times, the objects will appear with their natural clearness as if we were looking at them with the naked eye. We shall call this degree of natural clearness 1; and we shall express, in the following table, for each magnifying power, the clearness of the object by the thousand parts of the unit.

In the same table we shall insert all the diameter of the part of the object, which will be seen at once by each magnifying power.

But to judge of the magnifying power, we shall refer it, as is usual, to the distance of eight inches, so that the numbers marked for magnifying power will indicate how many times each object will be seen greater by the microscope than by the naked eye at the distance of eight inches.

This being settled, the first column A, of the table will mark the magnifying power; the second B, the distance between the object-glass and the first eye-glass; the third C, the degree of clearness; and the fourth D, the diameter of the portion of the object seen by the microscope.

A	B	C	D
25	0,781	0,800	0,115
50	1,562	0,400	0,064
100	3,125	0,200	0,035
150	4,687	0,133	0,025
200	6,250	0,100	0,109
250	7,812	0,080	0,015
300	9,375	0,066	0,013
350	10,937	0,057	0,011
400	12,500	0,050	0,010
450	14,062	0,044	0,009
500	15,625	0,040	0,008
600	18,750	8,033	0,007
700	21,875	0,029	0,006
800	25,000	0,025	0,005

A	B	C	D
900	28,125	0,022	0,004
1000	41,250	0,020	0,004
1200	37,500	0,016	0,003
1400	43,750	0,014	0,003
1600	50,000	0,013	0,003
1800	56,250	0,011	0,002
2000	62,500	0,010	0,002
2500	78,125	0,008	0,002
3000	93,750	0,007	0,001

As for the degree of clearness, we must also observe, it is not absolutely proportional to the numbers C in the table, but more to the square of these numbers, so that the clearness decreases much more than it is marked in the table. If you wish to magnify one thousand times, the degrees of clearness is not $\frac{1}{50}$ of the natural clearness, but only $\frac{1}{2500}$, but that degree of clearness is still pretty great in comparing it to the clearness of the full moon, which cannot exceed $\frac{1}{250000}$ of that of the sun; from which we see that the degree of clearness which corresponds to a magnifying power of 100 is still ten times greater than the clearness of the full moon, which will be sufficient for the objects we may wish to examine.

If we wished, however, to increase the magnifying power, we must illuminate the objects as we do when we employ the common microscopes; but it will not be necessary to go farther, because it is probable that a magnifying power of 300 or 400 times, which represents the objects distinctly, will discover a great deal more than the common microscopes with a magnifying power of some thousand times.

N. B.—This article is an extract from a work published at Petersburg in 1774, under the title of *Detailed Instructions to bring Telescopes of different kinds to the highest degree of perfection of which they are susceptible, deduced from the Dioptric Theory of Leonard Euler, and written in a way to assist workmen, by Nicolas Fuss; with the Description of a Microscope of the most perfect kind, capable of every degree of magnifying power.* Article 7, p. 75 to 83.

ART. IX.—*Remarks on Mons. Chevalier's Paper, and on the Memoir of Euler.* By DR GORING. Communicated by the Author.

A. IT will no doubt be thought that M. Chevalier has been somewhat tardy in his reply; but it must be recollected that, in order to answer me with effect, it was necessary for him to perfect his object-glasses, which he has even now scarcely accomplished, at least to the extent contemplated by him.

AA. Simplicity is, in my mind, a capital ingredient in all inventions and works of art. M. Chevalier has invented a kind of object-glass which will assuredly descend to posterity, for it is almost the only form in which achromatic object-glasses for engyscopes can be made with any sort of facility by moderately gifted artists; the radii of their inner surfaces being always rigorously the same, generate double aplanatic foci and a variable aberration, on which Mr Lister has founded an admirable system of correction. These advantages, however, were never contemplated or applied by M. Chevalier, and are the exclusive discovery of Mr Lister.

B. So effectually had M. Chevalier blinded his object-glasses, with respect to *lined proof-objects*, by an excessive reduction of their aperture, (owing to his ignorance of them at the time,) that had we been disposed to take an ungenerous advantage of his insensibility to the value of aperture, it would have been easy to have utterly destroyed the reputation of his instrument, (which could always have spoken for itself,) instead of which, we, with the greatest tenderness, opened the eyes of his unfortunate offspring, and made them see very well.

C. One of the quadruple combinations of this set was beautifully perfect; the sextuple system was well corrected for spherical aberration, but rather over-corrected in point of dispersion. I certainly do not think this defect of much consequence. It merely causes the outline of a transparent object to be of a bluish colour, instead of its natural tint. Under-correction gives an outline of red and orange, which I utterly detest. Nevertheless, in order that a false doctrine may not be deduced from this concession, which might be found exceedingly comfortable and convenient to slovenly opticians, viz. that if the spherical aber-

ration is well corrected, it is of small moment whether the chromatic is so or not.—I must observe that *the necessity for removing dispersion is never so sensible as when the spherical has been withdrawn*; for the latter covers and conceals the former, just as the taste of one sapid substance will cover that of another, or, speaking accurately, as a fog conceals the tints of a landscape.

This set of object-glasses was exquisitely centered and adjusted.

D. Proof-objects have been wretchedly abused by dishonest tradesmen, owing to the vast difference of facility of resolution which exists between specimens of the same kind, or nearly so, so that it has been well observed by Mr Lister, that the only safe way to determine between two microscopes, is to apply *the same identical scale to both*, under the same circumstances.

In illustration of this position, I shall observe, that I have seen a specimen of French podura of no more difficult resolution than the menelaus; yet not to be distinguished from the difficult English podura, by its outward and visible character. Even among the most difficult English podura there are frequently a few large coarse scales to be found of very easy demonstration. The same discrepancy is found among the other kinds of lined objects.

E. See my paper on monochromatic light.

F. This is a very excellent set of object-glasses for practical purposes, but neither of the sextuple compositions are so perfect as that which I shall next describe; in order to arrive at perfection, it will, I believe, be found necessary to have *fixed combinations*. My idea of a perfect achromatic microscope would be this, it should have only one achromatic eye-piece, and that to be as shallow as possible, all the different powers to be obtained by changing the object-glasses of which there should be, if only of the double kind, about half a dozen fixed compositions or systems ranging from two inches focus to the utmost shortness consistent with perfection; in short, the instrument would be pretty much like a compound microscope of the old plan, furnished with achromatic systems of object-glasses.

G. This set is about $\frac{1}{4}$ th of an inch focus, and has an
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gular aperture of fifty degrees or thereabouts. Each of its component lenses has some defect or another, which renders it unfit to act alone, or, in short, in any other manner than combined with the other two in the required order. The composition of the three is, however, *without fault or blemish of any kind*, it cannot be said that there is any *defect of centering or adjustment, or any spherical or chromatic aberration*, which the eye at least can detect; for that tendency to over-correction, both for sphericity and dispersion, which seems irremediable in object-glasses, embracing a very large pencil, is on this very minute scale *wholly insensible*, and so of course is the secondary spectrum, so that M. Chevalier has here contradicted himself by his own works, and produced that ultimatum which he insists upon being unattainable. Moreover the oblique pencil is well corrected.

It will be recollected that the aberrations of an ordinary uncorrected object-glass of the same focus and aperture are *faint* in comparison to those of longer foci, taking in a pencil of the same size, so that a common lens of $\frac{1}{10}$ th of an inch focus, and an aperture of thirty-six degrees, has the power (though with much confusion,) of showing all those lined objects which an achromatic of one inch focus and an equal aperture can show when raised to the same power, (let us suppose $\frac{1}{30}$ th of an inch). Therefore, if we wish to make a fair comparison between the performance of an achromatic object-glass and a common one, both should be of the same focus and angular aperture.

Though we may in some measure make a common compound microscope compete with an achromatic one, *having an object-glass of long focus*, by making that of the former *very deep*; yet when we get a perfect achromatic system of $\frac{1}{10}$ th of an inch focus, its superiority over a common $\frac{1}{10}$ th of an inch becomes nearly as manifest as that of an achromatic of one inch focus over a common one inch one.

M. Chevalier has now, I think, arrived at the shortest focus which it will be possible to use *upon opaque* objects. With regard to the performance of this system, I think it enough to say, that I have seen with it the two sets of diagonal lines on the true difficult *Picris brassica*, which I never saw before, with any sort of certainty, with any achromatic object-glass;

yet not so strongly marked as I have seen them with Mr Cuthbert's $\frac{3}{10}$ ths of an inch metals, *having an aperture of $\frac{3}{10}$ ths of an inch*; a striking proof, in my mind, of the superiority of the reflecting principle, for the advantage in point of power is three to one in favour of the refractor. However, every artist will eternally insist that there is nothing equal to *his own leather*, be it what it may. This set comes up to a power equal to that of a $\frac{1}{30}$ th of an inch lens *without any forcing*, and the vision is, with this amplification, certainly equal in point of distinctness to that of any single or compound magnifier, and superior in the article of achromatism, field of view, &c. The said set of object-glasses are in the possession of Mr Pritchard, Picket Street, Strand.

H. I have lately received an intimation from M. Chevalier, that he expects to effect a still greater reduction of the focus of his object-glasses, by forming an achromatic system, by combining two over-corrected object-glasses with a common lens, according to the plan laid down by me in my paper on facilitating the manufacture of engyscopic objectives. See this *Journal*, No. viii. p. 244.

I. It is not my intention to enter into any controversy here concerning the invention of achromatic object-glasses, which being a very valuable one, has of course plenty of fathers to it. It must be obvious, on the most superficial consideration, that the object-glasses of microscopes are merely modifications of those of telescopes, therefore, there was nothing to be invented about them.

We are perpetually annoyed with new *applications* and *adaptations* of old things, which are falsely termed new inventions; thus it is customary to style a steam-boat, a steam-carriage, steam-gun, a cotton-mill, &c. new inventions, though they are only so many ingenious and valuable applications of a steam-engine, highly creditable to those who have brought them into use.

Euler is undoubtedly the original adapter of achromatic object-glasses to the compound microscope. Their use as magnifiers, as eye-pieces, as well as for forming the image of the solar microscope, was, I think, originally pointed out by myself in the first treatise I wrote upon them; but all these ap-

plications are so very obvious, that the credit, (if any,) which may be attached to them, is scarcely worth having.

K. It would be easy to bring forward a considerable mass of evidence to prove that achromatic object-glasses for microscopes had been repeatedly tried and abandoned in this country many years back ; a circumstance of which I was perfectly aware before I employed Mr W. Tulley to construct them for me. Thus, in Adams on the microscope, a work published in 1787, is the following passage: "Achromatic glasses. How far this invention is applicable to the improvement of microscopes has not yet been ascertained ; and, indeed, from some few trials made, there is reason for supposing they cannot be successfully applied to microscopes with high powers ; so that this improvement is yet a *desideratum* in the construction of microscopes, and they may be considered as being yet far from their ultimate degree of perfection."—Pp. 48, 49.

In May 1807, the celebrated Mr Troughton employed Mr W. Tulley to make him a dozen achromatics of one inch focus for his astronomical circles, but rejected them as worse than common lenses, on account of their bad execution. It can, moreover, be most distinctly proved, that Mr Dollond has had a triple achromatic of one inch focus kicking about his shop for more than thirty years back. The reason that none of these object-glasses were brought to perfection was because *proof-objects*, were not discovered at the time of their fabrication, so that opticians had *no certain object to aim at*. I am convinced that the utility of achromatics would never have been recognized to this day ; nor would they, except by chance, have been constructed in an effective manner, had it not been for my own discovery of the peculiar property of the lined objects. In confirmation of which, I may observe, that any discovery of fresh proofs of still greater difficulty than those previously known, has been invariably found to produce fresh improvements in object-glasses and microscopes in general. Thus, when the brassica and podura were discovered ; opticians immediately found themselves under the necessity of extending the angular aperture of their object-glasses, and of combining two or three together, in order to give them the power of discerning the lines on these objects ; because it was

found no single object-glass could embrace a pencil of light of sufficient size to render them visible. I appeal to opticians whether telescopes have not been greatly improved since it has become the fashion to try them on double stars. Formerly they were considered good if they showed the moon and the planets well; but a much greater degree of perfection is now required of them, because they have to deal with what are to them proof-objects.

Had we remained to this day ignorant of proof-objects, opticians might certainly have made achromatic object-glasses for microscopes; but they would have been the poorest things imaginable, for the value of a large aperture would never have been felt, and when that of achromatics is reduced to a level with that commonly employed in the old compounds, their value or superior utility can hardly be appreciated, because the aberration of the common instruments is in this state invisible to ordinary eyes.

When I first took the subject in hand, I mentioned it to perhaps the first theoretical optician of which this country can boast. He was, moreover, in the habit of observing with the microscope, and the remark he made me was this: "Why, I always thought compound microscopes were achromatic," To which I answered, "that they might appear achromatic under certain circumstances, provided the object-glasses had an exceedingly small aperture, as was generally the case." Now, if such a man could form such an opinion, what may be expected from the vulgar?

L. This is the date I think of the first *effective* double-object glass. Though the error of the oblique pencil is so great in a glass of this description, if it has a sufficient aperture to show proof-objects, that all objects out of the pale of a few degrees from the centre of the field of view would be completely confused, it is this defect which renders a double object-glass unfit to be used, except in combination, unless its aperture is very much reduced.

M. In the autumn of 1823, Mr W. Tulley commenced his labours on my account, and about the 1st of March 1824, produced an object-glass of 0.333 focus and 0.2 of aperture; for from the very beginning Tulley always aimed at the utmost pos-

sible reduction of the focus. The next he made for me was of two-tenths of an inch focus, and 0.1 of aperture. The 0.933 was made afterwards at the suggestion of Mr Lister.

The said 0.333 focus object-glass was, I rather think, the first effective one which chanced to be made at least by design and principle, *for it was bottomed on proof-objects previously discovered with the single microscope*. However, I shall leave it to posterity to determine this point. The triple form is evidently that adapted to the microscope; for one alone well executed is capable of receiving an aperture quite sufficient for all ordinary purposes, and also for shewing *the majority of the lined objects*, which advantages it combines with a good correction of the oblique pencil. We do not want to be eternally looking at proof-objects any more than the astronomer does to be perpetually observing double stars and clusters of difficult resolution. A telescope may be a very good and valuable instrument, though it has not the power of showing *all* the latter bodies; and so may a microscope, though it should not be able to exhibit the more difficult lined objects.

I certainly do not like triple object-glasses in combination. There is a dimness in the vision of opaque objects caused by the loss of light from their numerous surfaces; but if they could be made *thin, and with their inner surfaces in contact and cemented together*, two of them would evidently be clearer than three double ones, (because two additional surfaces would be obliterated,) and quite as effective. Moreover, I am pretty sure that a triple object-glass highly over-corrected for sphericity and dispersion might be combined with a common lens for the purpose of increasing its power and angular aperture and improving its oblique pencils, and be equal in effect to two triple object-glasses of equivalent foci, with a far greater simplicity of construction.

The practice of piling Mount Pelion on Mount Ossa will never be resorted to, unless in a case of great necessity, by a real artist.

N. What a compliment is it to the reflecting engyscope to construct an achromatic refractor as nearly as possible in its form, by directing the rays at right angles by means of a rectangular prism; and this too purely on the ground of *the at-*

tainment of a more commodious method of observation. I certainly have seen many observers who prefer this form of the refracting instrument, though I do not admire it myself.

Remarks on Euler's Memoir.

O. In page 6 of the first article of Euler's memoir, "*Des verres objectifs délivrés de toute confusion*," speaking of triple achromatic object-glasses, he says, "they might also serve to improve microscopes by making them as small as possible, but then you must reverse the order of the *lenses*, by turning the third towards the object in its focus. To succeed better in the construction of them, it will be advisable to inclose each lens in a little cell, so that the distance between them may be a little varied, in order to discover, by experiment, the best disposition of the glasses relative to each other, for it is almost impossible to execute in practice all the prescribed measures, but a little change in their distance will be sufficient to remedy this defect."

When I consider this passage in conjunction with the curves given by Euler, I am almost certain that he had no other idea about the object-glass of an engyscope, but that *it was to be that of a very small telescope of a larger angle of aperture than usual, reversed or inverted*. The same notion I have found to prevail among other mathematicians, with whom I have conversed on the subject; and I must confess that in theory the doctrine seems plausible enough, for when a telescope is made to act upon a near object, say forty or fifty feet distant, (and be it observed that opticians like to try them best in this way, as they can be more certain of their quality than when made to perform with more parallel rays,) there is not a greater disproportion between the length of these conjugate foci than takes place in an engyscope, therefore the rule ought, at all events, to be good enough for practical purposes; nevertheless, I am convinced myself that it is far too rude and coarse; no optician could, I am sure, make a triple object glass for an engyscope by it to please the most ordinary customer, though it might do well enough for a mere magnifier.

I suppose, if it is conceded that the object-glass of a small telescope should, when turned the wrong way, constitute that of an engyscope, the object-glass of the latter must also, when turned the wrong way, form that of a telescope. Now we will screw this doctrine up to the sticking-place, and see if it will be found wanting or not when made to stand the brunt of facts.

The following are the results of trials with a $\frac{3}{10}$ th triple engyscopic object-glass used as a telescope. I must premise, that the said object-glass is achromatic and aplanatic, with an aperture of about $\frac{3}{10}$ of an inch, used as a microscope, but if the aperture is greater, the spherical aberration begins to preponderate in the *concave*. If the whole aperture $\frac{5}{10}$ is employed, it is very violent in the *concave*, and there is also a tendency to over-correction in point of colour. I mounted the said object-glass as a telescope, with an Huygenian eyepiece, which made it magnify nearly four times, and reversed the order of its lenses.

Whole aperture.—Spherical aberration very violent in the *convex*. There seems to be no difference which way it is turned towards the object.

Cut off to $\frac{4}{10}$, do. do.

Cut off to $\frac{3}{10}$, the same but weaker.

Cut off to $\frac{5}{10}$, spherical aberration, the same way as before, whether the object-glass is inverted or not. A little under correction in point of colour, now becomes manifest. This was before corrected by the strong spherical aberration in the convex lenses, which is now, by the reduction of the aperture, considerably weakened in intensity.

Aperture $\frac{1}{10}$.—Aberration greatly reduced, but still the same way as before; that of a plano-convex lens of the same focus and aperture, with its covered side towards the object, scarce to be distinguished now from that of the achromatic.

I may observe, that all the triple achromatics I have ever tried give much the same results. The curves of the $\frac{1}{10}$ object-glass are,

Radius of 1st surface,	0.825	} convex, (Crown)
2d, ———	0.525	

Radius of 3d surface,	0.5	} concave, (Flint)
4th, ———	0.5	
5th, ———	0.575	} convex, (Dutch Plate)
6th, ———	0.575	
Specific gravity of crown,	2.527	}
Thickness of do.	0.15	
Specific gravity of flint,	3.627	}
Thickness of do.	0.164	
Specific gravity of Dutch,	2.519	}
Thickness of do.	0.175	

Any mathematician may see that there is in this object-glass a vast excess of aberration in the convex lenses, beyond what would be required in a telescope, which ever way the rays pass through the lenses.

I once procured the object-glass of a small perspective of little more than four inches focus, and somewhat more than an inch of aperture, and converted it into the object-glass of a long engyscope by reversing it. I regret that I did not make notes of my observations with it, as I cannot now obtain it again; I only recollect that *it did not answer at all*, and was *not even achromatic* used in this way. If I can rely on my memory, this object-glass, used in its natural way as a telescope, had an excess of aberration in its convex lenses, (at least with its whole aperture,)—a circumstance which should have operated much in its favour.

I conceive, therefore, that Euler has done little or nothing beyond pointing out the application of achromatic glasses to form the images of engyscopes, both because he has selected an angle of aperture much too small for efficiency *even on objects not of lined kind*, and because his system of reversing the order of the curves of an achromatic, in order to adapt it to diverging rays, will be found utterly inadequate to the purpose. The affair will not be got over so easily I am sure, but will prove a confounded tough job for the proudest mathematician in Europe. The talents of Euler were perhaps little inferior to those of any man living or dead. Had he taken up the subject in the right point of view he would doubtless have succeeded.

A double object-glass agrees rather better with his theory

than a triple one, that is to say, it performs *better* upon parallel rays when inverted than it does when its concave lens is presented to them to form a telescope; but to obtain this approximation it must be very thin, and have a very small aperture, and is even then a long way off from its true figure.

As metals are very simple in their action compared with achromatic glasses, I always like to put a case with regard to them in order to settle any theoretical point. Accordingly, I have tried a set of beautifully perfect metals belonging to an Amician reflecting engyscope, inverted as miniature Newtonian telescopes, and found their figure a *great deal too spherical* for parallel rays, especially those of the large angles of aperture.

Had Euler's theory been correct, they ought to have performed very well as minute telescopes, because their figure should, in this case, have been *parabolical*, instead of which it is much nearer the sphere, and, as I presume, *elliptical*, which harmonizes perfectly with the other trials; for the difference between the ellipse and the parabola is equivalent to that between the condition of an object-glass perfect in its action on parallel rays, and another which has too much spherical aberration in its convex lenses to be so. The said metals are, however, not so much out in point of figure as a triple object-glass is, but about as much as a double one, as far as I can guess. The theory of achromatic glasses for diverging rays, is then, I am persuaded, an untried field, in which no mathematician has as yet distinguished himself.* I shall probably be told that a theory would be of no use if we had it, because *artists would not work to it*. This is true with regard to the more minute glasses; but very beautiful and useful engyscopes may be made on a large scale, having object-glasses of so long a focus as four and even six inches, which also act very strikingly in the solar microscope; and a theory could

* Money is said to make every thing go; to be at the bottom of every thing; and to purchase every thing, (if in sufficient quantity.) Thus all men and all women have their price. When I get rich I shall try if the offer of a good round sum will stimulate the men of numbers and quantities to undertake the theory of a thick aplanatic object-glass for diverging rays.

always be tried and executed on this scale, which would not strain the powers of a *real artist* too much.

The principal opticians in Europe have made achromatic object-glasses for engyscopes for some years, and are immeasurably in advance of the mathematicians on this subject. If the latter gentlemen do not choose to turn their attention to it, I presume they will be content to retire into the shade; to abate somewhat of their magnificent pretensions to infallibility and exactitude; and to admit that for once analysis has been beaten by experiment, at least in the *pitiful, pimping, insignificant*, article of the object-glass of a compound microscope, which being such a *mere trifle* cannot of course reflect any disgrace on their incapacity to handle it.

———"tractata quæque nitescere posse
"Desperant, relinquunt."——

LAMBETH, *June 30, 1831.*

ART. X.—*On some properties in Achromatic Object-glasses applicable to the improvement of the Microscope.* By JOSEPH JACKSON LISTER, Esq. (Concluded from No. ix. page 180.)

OF several purposes to which the particulars just given seem applicable, I must at present confine myself to the most obvious one. They furnish the means of destroying with the utmost ease both aberrations in a large focal pencil, and of thus surmounting what has been hitherto the chief obstacle to the perfection of the microscope. And when it is considered that the curves of its diminutive object-glasses have required to be at least as exactly proportioned as those of a large telescope, to give the image of a bright point equally sharp and colourless, and that any change made to correct one aberration was liable to disturb the other, some idea may be formed of what the amount of that obstacle must have been. It will however be evident, that if any object-glass is but made achromatic, with its lenses truly worked and cemented so that their axes coincide, it may with certainty be connected with another possessing the same requisites and of suitable focus, so that the

combination shall be free from spherical error also in the centre of its field. For this the rays have only to be received by the front glass B, Fig. 6, Plate I. from its shorter aplanatic focus f , and transmitted in the direction of the longer correct pencil f A of the other glass A. It is desirable that the latter pencil should neither converge to a very short focus, nor be more than very slightly, if at all, divergent; and a little attention at first to the kind of glass used, will keep it within this range, the denser flint being suited to the glasses of shorter focus and larger angle of aperture.

The adjustment for the microscope is then perfected, if necessary, by slightly varying the distance between the object-glasses; and after that is done, the length of the tube which carries the eye-pieces may be altered greatly without disturbing the correction; opposite errors which balance each other being produced by the change.

If the two glasses, which in the diagram are drawn as at some distance apart, are brought nearer together, (if the place of A for instance, is carried to the dotted figure,) the rays transmitted by B in the direction of the longer aplanatic pencil of A, will plainly be derived from some point ($\approx$) more distant than f'' , and lying between the aplanatic foci of B; therefore (according to what has been stated) this glass, and consequently the combination, will then be spherically over-corrected. If on the other hand the distance between A and B is increased, the opposite effects are of course produced.

In combining several glasses together, it is often convenient to transmit an under-corrected pencil from the front glass, and to counteract its error by over-correction in the middle one.

Slight errors in colour may in the same manner be destroyed by opposite ones; and on the principles described, we not only acquire fine correction for the central ray, but by the opposite effects at the two foci on the transverse pencil, all coma can be destroyed, and the whole field rendered beautifully flat and distinct.

The occurrence of two good combinations among the numerous ones of Utzschneider's glasses will now appear only what might be expected; and more are to be obtained from them by varying the distance of the glasses from each other.

I have very lately seen one of Chevalier's combinations of three glasses, each about 0.4 inch in focus, inferior indeed to these, and of contracted aperture, but which I ought to notice because its image is pretty well corrected with the glasses in the order given to them by the maker. At the same time, I do not suppose him to be acquainted with the principle which constitutes the key to the effect; otherwise he would hardly have failed to apply it more effectually.

The achromatic meniscus that has been described, enters well into combination, and may perhaps be useful as the front glass of three, but its power is small in proportion to its curves. It admits too of being cemented at the back of an achromatic plano-convex; and they may make together a powerful compound glass to go before another.

Though the plano-concave form has been proposed for the flint lens, in the larger glasses it might perhaps occasionally be relinquished with benefit.

Some attention has yet to be given to obtain the best effect from combination; so that with the largest pencil that may be found desirable, and sufficient clear space before the front of the glass, the field may be aplanatic, and the focus short. Already, however, the three first plano-convex glasses that have been made for me by Tulley, only for preliminary experiments, the shortest of them of 0.7 inch focus, have produced at an aperture of 50° , the most distinct microscopic vision that I have yet met with; and I anticipate no serious impediment to the carrying defining power much farther.

These statements are intended only as a notice, for practical purposes and wholly detached from theory, of facts that have been in part very recently ascertained. In investigating them I have depended chiefly on magnified measurements and diagrams, which, though not strictly correct, may perhaps be as well adapted as other far more exact but difficult methods, for constructing the minute and complex object-glass of a microscope. It is to this that the observations made particularly apply; and should they bring more within reach than previously, the requisites which have been enumerated, I trust they may not be unacceptable to the enlightened optician; es-

pecially if in this department he unite, like Tulley, the zeal of the amateur with the skill of the superior artist.

I intend soon to put to the test of experiment, whether or not the principle of the two aplanatic pencils may be applied to telescopes, in cases in which it is requisite to restrict their length, so as to enlarge their aperture with a corresponding increase of light and distinctness. In the meantime, it would give me pleasure to see that principle demonstrated, if it deserve it, by some abler hand than mine, and treated in a more rigorous manner than my own limited acquaintance with mathematical science qualifies me to undertake.

(Note A.) The blue down of the Menelaus and the white of the Cabbage Butterfly (*Morpho Menelaus* and *Pieris Brassicæ*) well deserve the place they have acquired as standard tests, especially on account of the respective characters of their transverse tracings. Some of the small oval scales from the body of the Twenty-plumed Moth (*Alucita hexadactyla*) are among the closest and most elegant in their lines that I have seen; others are very easily resolvable, and the down has such diversity of form as often to afford within a short space a ready variety of excellent tests. The same is to be said of the scales of *Podura plumbea*, of which all are difficult, and some seem to defy all powers of definition.

It may be observed, that with most objects there is such difference between individual specimens of the same kind, that in general the only safe way to determine between two good microscopes, is to apply the same specimen to both, under the same circumstances of power and light.—*Phil. Trans.* 1830.

ART. XI.—*Account of the Aurora Borealis seen in Roxburghshire on the 5th of October 1830.* By Mr W. LAIDLAW, Kaeside.

ON Friday evening, the 17th September, an uncommon form of the aurora borealis was observed all over the country. It was in the shape of a luminous white arch, and stretched from W. by S., to E. by N. nearly, ending on the E. in a pencil-formed point, and was accompanied by pale streamers in the

north horizon; and continued visible from twenty to thirty minutes.

This evening, (October 5,) I was called to observe an appearance that seemed at first to be nearly similar, though it was more bright and better defined. When first seen it passed exactly overhead, and in a direction perhaps nearly the same as the former; it was very bright, and white on the N. edge, and better defined than on the S., on which side it in some parts shaded off into the sky. On the west end it sunk bright below the horizon, but on the east, where it approached the moon, it ended like the former in a tail or brush-shaped point, before it reached the horizon,—perhaps about 20° above it.

Having no instrument to measure the breadth, I compared it with the distances of some stars, and found it equal to the longest diagonal of the square of the Great Bear,—wider in the E. and narrower in the W. After looking at it occasionally for about twenty minutes, I began to be aware that the arch was making a progress towards the south, and had already gone over a space equal to its own breadth. I remarked likewise, that its extremities were comparatively stationary; or that it revolved on its horizontal diameter, and at thirty minutes after eight, the arch through which it moved was, as near as I could judge by the eye, equal to three times its breadth. I now regretted still more the want of an instrument to measure angles; for, while lying upon my back to observe the curvature, I noticed that the bearing of the extremities was not due E. and W.; but that a line from the Pole star formed an acute angle with the eastern half, and I began to suspect that the centre of the arch might be the magnetic pole. At this time the brightness began to fade, and a longitudinal fissure appeared in the E. quarter. At nine o'clock, the whole arch had become broader and considerably less bright; and in a short time after it very suddenly broke up into fragments, which I soon observed had a motion, following one another in the line of the arch from E. to W. My attention was now strongly directed to this movement, which I never before noticed in any of these arches.

The appearance which this now assumed was very interesting. Whether it was owing to the moon being in that part of the hea-

vens that directed my attention to the E., I am uncertain; but I first observed that in the eastern part the separated portions or nebulæ had assumed a striated or rather crinkled appearance, the lines of each individual nebula pointing S. E., even while they moved all westward, following one another in an order parallel as to each other, but obliquely as to their position on the arch, so that to a careless observer the first general form of the arch would have seemed to be preserved:—only that at, and on a little on each side of the meridian, the general continuous line of the arch was interrupted; and here an interesting change took place in the direction of the striæ of the nebulæ, or rather in the order in which they appeared when farther east:—For, on crossing the meridian, they veered to the right, and formed on the western part of the arch, with the striated appearance pointing S. W. During the time that this movement continued, coruscations were apparent, and sudden alternate disappearances and reappearances of the brightness, similar to what is often observed in the common streamers; the striated divisions not being always, nor throughout the whole length of the arch, distinctly separated from each other. These coruscations were most apparent in the eastern quarter, although they sometimes flashed from one end of the arch to the other, and always from east to west.

The motions of the striated divisions had the apparent velocity of clouds in a gale of wind, but the shifting and flashing was similar to that of the streamers usually seen in the north. I have since learned from two different persons, one in this neighbourhood and the other in Liddesdale, that there were two arches that evening, one a little after sunset, which was not so bright and soon disappeared; and that the second flashed out at once bright into the sky.

It has since occurred to me that the motion of the striated portions of the arch from E. to W., and which seemed so singular, would be best described by referring it to the motion of a wheel with radiated teeth. The motion of such seen in perspective, would exactly represent the extraordinary appearance attempted to be described.

ART. XII.—On the Mean Temperature of THIRTY-FOUR different places in the State of New York, for 1830.

IN the sixteenth No. of this *Journal*, and in Nos. ii. and vii. of our New Series, we have given abstracts of the “Returns of Meteorological Observations made to the Regents of the University of the State of New York for 1826, 1828, and 1829.” We are now enabled to present our readers with an abstract of the same valuable returns for 1830. These returns have been made by no fewer than 42 academies; but only 34 of them are complete.

The observations seem to have been made at the same hours as formerly, viz. 6^h A. M. 3^h P. M., and an hour after sunset.

In No. vii. p. 78, we have given the positions of the places where observations were made in 1829. The following Table contains the positions of the academies not in that list, which have made observations for 1830.

	N. Lat.	W. Long.	Elevation above tide.
Canajoharie, -	42° 53'	74° 35'	284
Cayuga, aurora, -	42 45	76 40	412
Cortland Homer, -	42 38	76 11	1096
Delaware, - -	42 17	74 54	1384
Fairfield, - -	43 6	74 54	1185
Fredonia, Chautangue Co.	42 25	79 24	645
Gouverneur High School,	44 25	75 35	400
Ithaca, - -	42 26	76 30	417
Kinderhook, - -	42 24	73 45	125
Lewiston, - -	43 9	79 10	280
Mount-Pleasant, -	41 7	74 16	
Plattsburg, - -	44 42	73 23	105
Redhook, - -	42 2	73 56	
Rochester High School,	43 5	77 45	506
Union Ellesburgh, -	43 46	76 11	

In 1826 the mean temp. of ten places was 49°. 4

In 1828 the mean temp. of twenty-three places was 49.99

In 1829 the mean temp. of twenty-eight places was 46.45

In 1830 the mean temp. of thirty-four places was 48.15

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The following table contains the mean monthly temperature of the thirty-four places above-mentioned, the annual mean temperature, the annual range, and the highest and lowest during the year.

	Jan.	Feb.	March.	April.	May.	June.
Albany,	24.56	24.82	38.05	56.02	59.27	65.98
Auburn,	22.54	23.62	34.55	50.63	53.90	62.06
Cambridge, Wash.	23.73	18.79	34.52	49.69	54.19	59.57
Canajoharie,	16.60	20.80	31.78	50.17	56.14	63.60
Canandaigua,	22.50	24.86	35.20	52.02	53.25	63.42
Cayuga, -	26.82	27.53	36.56	49.18	53.61	63.08
Cherry Valley,	19.02	20.19	31.17	48.56	53.92	61.27
Clinton, -	30.88	27.20	37.97	45.94	51.56	63.02
Delaware, -	19.49	22.71	33.62	45.86	50.81	57.88
Dutchess, -	26.47	26.68	38.94	55.68	62.33	69.83
Erasmus-Hall,	28.90	30.83	40.84	50.96	58.16	67.35
Franklin, -	20.11	24.79	33.69	48.71	52.35	61.14
Fredonia, -	25.89	26.90	38.23	53.35	56.21	62.18
Hamilton, -	19.16	21.93	31.51	50.38	52.08	60.10
Hartwick, -	22.45	25.42	35.85	51.25	52.75	59.65
Ithaca, -	25.39	29.75	37.86	52.89	55.98	64.15
Kinderhook,	22.01	22.59	37.17	51.71	54.47	63.58
Kingston, -	25.28	25.11	40.38	56.46	61.76	66.19
Lansingburgh,	22.53	23.36	36.15	54.45	58.46	65.98
Lowville, -	15.35	17.64	31.20	49.76	52.91	60.00
Middlebury,	23.16	26.51	36.01	50.79	52.86	61.05
Montgomery,	26.98	27.29	39.00	53.93	58.71	68.58
Newburgh,	25.68	24.48	37.00	50.50	57.82	65.11
North Salem,	26.87	25.54	38.65	51.22	57.43	65.25
Oxford, -	19.66	23.40	34.30	48.84	54.59	63.13
Pompey, -	18.13	21.82	32.54	48.75	51.56	59.48
Redhook, -	23.44	23.94	37.18	51.10	52.62	65.36
Rochester H. School	22.38	25.43	35.40	54.34	54.68	62.80
St Lawrence,	13.01	18.24	31.45	51.10	53.65	62.03
Union, -	19.89	24.35	36.25	55.90	51.47	56.54
Union-Hall,	29.21	28.85	39.31	50.29	56.15	66.90
Utica, -	20.08	22.26	33.95	51.31	53.13	61.40
Washington,	19.64	19.73	33.78	49.37	51.09	63.73
Seminary of G & O C	19.37	20.87	33.20	49.73	51.88	60.09

	July.	Aug.	Sept.	Oct.	Nov.	Dec.
Albany, -	73.86	70.52	61.82	52.39	46.67	33.87
Auburn, -	71.41	65.75	57.32	50.89	44.52	31.30
Cambridge, Wash.	70.73	67.57	63.40	52.30	47.63	33.06
Canajoharie,	72.84	67.77	57.71	47.88	41.97	31.10
Canandaigua,	71.83	65.49	56.44	51.58	43.35	30.72
Cayuga, -	69.97	67.08	60.32	56.60	48.57	34.32
Cherry-Valley,	70.32	62.41	57.07	47.79	41.58	28.78
Clinton, -	70.47	69.17	61.40	53.71	48.69	37.86
Delaware, -	71.08	66.95	59.14	51.23	44.09	27.03
Dutchess, -	77.57	73.37	64.37	55.36	48.28	33.86
Erasmus-Hall,	75.26	70.66	63.47	55.28	50.46	38.23
Franklin, -	73.12	64.65	56.69	48.62	43.40	29.30
Fredonia, -	73.57	67.76	58.71	52.88	46.93	32.30
Hamilton, -	69.68	65.34	55.80	50.03	43.44	30.50
Hartwick, -	69.32	64.26	56.96	48.21	43.50	30.55
Ithaca, -	73.33	66.16	58.88	51.78	46.66	33.08
Kinderhook,	72.36	69.00	59.46	51.25	43.75	31.56
Kingston, -	73.39	71.51	62.10	52.91	46.98	35.67
Lansingburgh,	73.76	71.01	60.68	51.35	45.29	32.65
Lowville, -	70.47	64.42	54.75	48.35	42.63	27.70
Middlebury,	70.23	65.30	58.38	52.10	45.75	32.40
Montgomery,	75.52	74.52	63.48	57.15	47.53	34.73
Newburgh,	72.37	69.47	59.93	59.01	47.06	34.68
North Salem,	71.85	69.11	60.64	51.04	47.59	34.44
Oxford, -	70.83	64.85	58.60	48.08	41.12	31.28
Pompey, -	69.62	63.72	54.72	47.23	41.64	27.01
Redhook, -	70.12	72.34	62.44	51.98	45.42	33.27
Rochester H. School	73.44	68.87	59.66	54.99	46.60	32.71
St Lawrence,	70.28	65.51	55.72	47.07	40.45	26.44
Union, -	67.59	59.49	54.15	46.59	43.69	25.38
Union-Hall,	74.16	70.12	57.63	53.96	48.21	35.37
Utica, -	70.26	64.75	56.22	48.25	44.49	31.22
Washington,	70.18	65.46	56.90	46.25	44.41	31.78
Seminary of G & O C	69.73	63.30	55.48	49.51	42.62	29.59

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	Ann. Mean.	Highest.	Lowest.	Ann. Range.	Great. mo. Range
Albany, -	50.65	97	—12	109	67
Auburn, -	47.37	96	— 4	100	56
Cambridge, Wash.	47.92	93	—25	118	85
Canajoharie, -	46.53	97	—16	113	60
Canandaigua, -	48.86	93	— 8	101	55
Cayuga, - -	49.47	94	0	94	51
Cherry-Valley, -	45.17	92	—22	114	72
Clinton, - -	49.82	93	2	91	51
Delaware, -	44.99	93	—17	110	
Dutchess, -	52.73	100	—10	110	71
Erasmus-Hall, -	52.53	93	4	89	54
Franklin, - -	46.38	90	— 7	97	60
Fredonia, - -	49.57	97	— 9	106	61
Hamilton, - -	45.87	95	—20	115	74
Hartwick, - -	46.68	91	—17	108	77
Ithaca, - -	49.67	96	— 1	97	60
Kinderhook, -	48.24	94	—14	108	70
Kingston, -	51.47	93	—13	106	71
Lansingburgh, -	49.64	98	—20	118	73
Lowville, -	44.60	93	—33	126	77
Middlebury, -	47.82	94	— 7	101	67
Montgomery, -	52.03	104	—12	116	68
Newburgh, -	50.25	94	—10	104	57
North Salem, -	49.96	97	—14	111	69
Oxford, - -	46.55	93	—15	108	70
Pompey, - -	44.68	90	—11	101	64
Redhook, - -	49.10	90	—10	100	69
Rochester H. School,	49.27	98	— 5	103	55
St Lawrence, -	44.58	90	—27	117	78
Union, - -	45.94	90	—23	113	77
Union-Hall, -	51.05	100	0	100	55
Utica, - -	46.44	90	—17	107	85
Washington, -	46.02	88	—24	112	73
Seminary of G. & O. C.,	45.45	91	—22	113	70

The following Table shows the quantity of rain and snow which fell in the state of New York in 1830.

	In.		In.
Albany, -	41.85	Kingston, -	40.15
Auburn, -	37.88	Lausburgh, -	40.67
Cambridge, Washington	35.10	Lowville, -	36.66
Canandaigua, -	36.60	Middlebury, -	38.50
Cayuga, -	37.11	Montgomery, -	40.99
Cherry-Valley, -	45.05	Newburgh, -	34.83
Clinton, -	46.65	North Salem, -	43.37
Delaware, -	22.55	Oxford, -	33.79
Dutchess, -	46.36	Pompey, -	30.06
Erasmus-Hall, -	53.47	Redhook, -	43.00
Franklin, -	36.15	Rochester High School,	34.94
Fredonia, -	33.93	Union, -	26.09
Hamilton, -	42.71	Union-Hall, -	43.32
Hartwick, -	41.59	Utica, -	46.19
Hudson, -	39.77	Seminary Gen. and	
Ithaca, -	35.61	Oneida Con. -	41.59
Kinderhook, -	36.92		

	Inches.
In 1826 the mean rain of nine places was	36.34
In 1828 the mean rain of twenty-five places was	36.74
In 1829 the mean rain of twenty-five places was	34.88
In 1830 the mean rain of thirty-two places was	38.86

Mean, 36.70

MISCELLANEOUS OBSERVATIONS.

Aurora Borealis noticed.

January 24, at Canajoharie academy, Utica.

Jan. 27, at Lowville.

Feb. 16, at Pompey.

Feb. 18, at Pompey, St Lawrence, Utica, Cazenovia.

Feb. 19, Albany, Canajoharie, Delaware, Kinderhook, North-Salem, Pompey, Utica.

Feb. 23, at Canajoharie. Segment large and luminous. Light in the west at half-past 8, like twilight: Lowville, North Salem, Utica.

March 15, at Albany, Canajoharie. Exhibiting the appearance of day break half an hour before sunrise: Franklin, Low-

ville, Middlebury, Pompey, St Lawrence. Very brilliant, Utica, Cazenovia.

March 16, Union.

March 28, Albany, Pompey.

March 31, at 10 P. M. a single column of light, a little west of north, extending up at right angles with the horizon, and subtending an angle of about 25 degrees, Franklin.

April 15, at Albany, Lowville, Pompey, St Lawrence, Utica.

April 19, from 8 P. M. till nearly midnight, the whole northern horizon presented at times, streams of light, which rose, enlarged and disappeared in succession, sometimes extending almost to the zenith; at others, rising only a few degrees and usually succeeded by a dark hazy appearance. The light from this source was sometimes as much as the moon affords, three or four days after conjunction, Auburn. Very brilliant: Cambridge, (Washington,) Canajoharie, Cayuga. At 9 P. M. a broad column of light, perpendicular to the horizon, and extending up about 45 degrees; the whole was so brilliant that the shadows of objects were distinctly seen on the north side of buildings. About midnight it was still more brilliant: Franklin. Brilliant: Hudson, Lansingburgh. Nearly one-third of the horizon illuminated during the night. The highest part of the illuminated segment extending to the height of 45 degrees; lively coruscations and various hues of great beauty especially a reddish tinge of vast extent, which appeared at 9 o'clock: Lowville. Very brilliant: Oxford, Pompey, Rochester, Union, Cazenovia. Very beautiful with high coruscations: Utica.

April 20, brilliant: Cayuga, Delaware, Kinderhook.

April 21, at Canajoharie.

April 25, at Rochester.

April 28, at Lowville.

May 2, brilliant, 40 degrees above the horizon, 8 P. M., Lewiston, St Lawrence.

May 4, at 10 P. M. at Auburn, Lansingburgh, Lowville, Oxford, Pompey, St Lawrence. Very bright: Utica.

May 9, at Canajoharie, Hartwick.

May 10, at Kinderhook.

May 11, at St Lawrence.

May 13, at Delaware.

May 14, at Canajoharie, Hartwick, Pompey, St Lawrence, Utica.

May 15, at Delaware, Kinderhook, St Lawrence, Union, Cazenovia. Very bright, Utica.

May 19, at Fredonia.

May 22, at Union.

June 9, at Middlebury.

June 10, between 8 and 9 P. M. an arc across the heavens, Albany. Very brilliant, and continued all night Auburn Luminous arch: Canajoharie. Very brilliant: Erasmus-Hall, Fredonia, Hartwick. A very splendid A. B.; the light so bright as to cast distinct shadows, Mount-Pleasant, Kinderhook. Very brilliant: Lewiston. Very beautiful, and extending from the east to the west and widening to the north horizon: Lowville. A beautiful arch: Oxford, Pompey, St Lawrence. Uncommon A. B.: Utica.

June 11, splendid arch: Dutchess, St Lawrence, Utica.

June 16, at St Lawrence.

June 17, at Hudson.

June 18, at Lewiston, Utica.

June 29, at Hartwick, Delaware.

July 7, at Lewiston.

July 14, at Albany, Canajoharie, Franklin. Bright luminous arch a little south of the zenith, from E. to W., descending to the south: Hartwick, Delaware. At 10 o'clock very bright: Ithaca. Very brilliant: Lansingburgh: Ibid: Lowville, Middlebury, Union-Hall, Utica.

July 15, at Erasmus-Hall. Noticed between 8 and 12 P. M. At first resembling a bright cloud in the form of the segment of a circle; the crown being about 30 degrees above the horizon, exhibiting for several hours a steady light. About 12 it rose in distinct and brilliant spires, shooting towards the zenith. From 9 to 10 were seen occasionally, beams of light shooting from two points, a few degrees east and west of the luminous segment, uniting and forming a belt at right angles with the galaxy, and resembling it in width and appearance, except much more luminous. This moved towards the south, and after passing the zenith, separated and disappeared: Fredonia.

July 21, at Lowville.

July 28, at Fredonia.

August 8, at Lewiston.

August 10, at Pompey, Utica.

August 11, at Franklin.

August 12, at Auburn, Utica.

August 13, at Auburn, Hamilton.

August 19, at Pompey, Union, Cazenovia. Uncommonly beautiful : Utica.

August 20, at Lowville, Pompey, Cazenovia, Utica. On the evening of the 20th the Aurora Borealis presented an unusual appearance. At half-past 9 the flashes of light, apparently proceeding from a dense cloud in the horizon, shot up very high in forms resembling pencils of rays. At 10, a broad arch, resting in the horizon, in nearly the western point, rose between Arcturus and Ursa Major, and extending over the whole heavens, a little north of the zenith, terminated about 20 degrees above the eastern horizon. Between this arch and the polar star, stood a row of perpendicular columns of light, with uniform bases, in a right line from Arcturus to the polar star. There were sixteen pillars on the inside of the arch, and one on the outside below Arcturus. They maintained their position for nearly half an hour, and gradually ascending, in the same regular order, grew fainter and disappeared. The flashes in the horizon still continued very light, but not distinctly defined : Utica.

August 21, at Hamilton.

August 25, at Lowville, St Lawrence.

August 26, continued all night : Auburn, Lowville, Pompey, Rochester, Union, Utica.

August 28, at Hartwick.

August 29, at Rochester.

Sept. 5, at Rochester.

Sept. 7, low but regularly arched, not above 20 degrees above the horizon at 8 o'clock, but by the interposition of vapour between the top of the arch and the horizon, forming a perfect segment of a circle at half past 8, two very bright lights, extending from the top of the arch to 50 degrees : Lewiston.

Sept. 9, at Fredonia, Utica.

Sept. 10, at Utica.

Sept. 11, at St Lawrence.

Sept. 15, at Albany. Very brilliant, in columns springing from an arch with prismatic colours, resembling those produced by finely striated metal : Pompey.

Sept. 16, at North-Salem.

Sept. 17, at Pompey, St Lawrence, Utica.

Oct. 5, at Utica.

Oct. 6, at Auburn, Canajoharie. Brilliant : Lowville, North Salem, Plattsburgh, Pompey, St Lawrence, Utica.

Oct. 7, at Auburn, Canajoharie, North Salem, Pompey, St Lawrence, Union, Utica.

Oct. 8, North Salem.

Oct. 9, at Auburn, Fredonia.

Oct. 10, at Fredonia, Lowville.

Oct. 11, at Utica.

Oct. 13, at Hartwick.

Oct. 14, at Pompey, Utica. Very brilliant : St Lawrence.

Oct. 15, at Lowville.

Oct. 16, at Lowville.

Oct. 17, at Lowville, Pompey, Utica.

Oct. 27, at Auburn.

Oct. 28, at Fredonia, Pompey. Very brilliant over the whole north, and extending to the zenith, and there having a fiery red appearance. Large red belts were occasionally seen streaming from the west, towards the zenith, and extending in one instance nearly to the east, spanning the chord of an arch of about 160 degrees, and about 8 or 10 degrees broad : St Lawrence, Union.

Nov. 9, at Pompey.

Nov. 19, at Hartwick.

Nov. 20, very brilliant : Canandaigua, Fredonia, Middlebury, Pompey, Utica.

Nov. 21, at Canandaigua.

Dec. 6, at Cazenovia.

Dec. 7, at Lowville. Very brilliant : Plattsburgh, Pompey, Utica.

Dec. 10, very brilliant : Dutchess, Hartwick, Plattsburgh.

Dec. 11, The following description gives but a faint, al-

though generally correct, idea of the splendid appearance observed this evening, between 9 and 10 o'clock :

“Early on Saturday evening an Aurora appeared in the north, stretching round from seven to eight points of the compass, and from nearly north-west to nearly north-east. The principal light was as usual in the form of an arch, the centre of which was directly under the north star. There was a general suffusion of light over the arch, and extending twenty or thirty degrees above it. The space beneath the arch, in the early part of the evening, was filled with a dark cloud. At about half past 6 o'clock a row of bright pillars or columns rose from the arch, and extended to a great height above it, and some of them nearly as high as the north star ; those over the north-western limb of the arch were slightly tinged with redness, and all the others were perfectly white. These pillars or columns soon disappeared and were succeeded by others ; they generally rose over one limb of the first arch, and gradually extended to the other. Between six and seven o'clock, the dark cloud beneath the arch rose and spread over a great part of the sky, and for some time entirely obscured the Aurora. At about eight o'clock some clouds had passed away, and a considerable portion of the upper part of the northern sky had become clear again, and a row of bright white pillars or columns rose from or through the dark clouds beneath, and extended far beyond the north star.—These pillars or columns remained but a short time, and the whole northern sky was again obscured by other clouds, and so continued through the greater part of the night. Through the intervals between the clouds or the thin places in them, the light of the Aurora was frequently seen in bright white spots over the northern part of the sky. The Aurora continued throughout the night, and between three and five o'clock on Sunday morning, the sky had become clear and the Aurora was quite bright :” Albany. Very brilliant : Auburn. Do. Dutchess. Very brilliant, illuminating the horizon and issuing several perpendicular columns, extending in a direction towards the zenith, from 15 to 20 degrees. The appearance was not unlike that occasioned by a very extensive conflagration in the city of New-York, Erasmus-Hall, Hartwick. Very brilliant : Lansingburgh,

Lewiston. There appeared two ranges in the north, one above the other, and though irregular, yet in their main direction, parallel, and resembling two sublime ranges of mountains with dark sides and slight summits of immense height, from which the brilliant coruscations, were, by the imagination, easily converted into volcanoes. In the horizon, the light extended from the south-east, almost to the west, and the currents and flashes of light rose from the north horizon to the zenith, and thence descended half way to the south horizon: Lowville, North-Salem, Plattsburgh, Rochester. Very brilliant: St Lawrence, Union, Utica.

Dec. 12, very brilliant, Auburn. Do. Dutchess, Franklin, Fredonia. Do. Ithaca, Lansingburgh, Lewiston, Middlebury, North-Salem, Plattsburgh, Pompey, St Lawrence, Utica.

This year has been remarkable for the frequency and often the brilliancy of the Aurora Borealis. In every instance, so far as observations have been made at this place, the phenomenon has presented a dark, dense, and regularly defined cloud, lying in the northern horizon, and extending far to the east and west, apparently a ground-work from which the light proceeds. On some evenings this cloud is above the horizon at the first appearance of the flashes of light; in others it is not visible until after the light has been observed for some hours, and when the flashes have continued late in the night, the cloud ascends above the horizon and the brilliancy of the light diminishes. In one instance, on the evening of the 11th of December, after the cloud had risen ten or fifteen degrees above a clear horizon, a second cloud appeared, from which the flashes of light were distinctly seen shooting to the upper one: Utica.

ART. XIII.—*Remarks on Mr Potter's Paper "on the Specific heats of Metals."* By JAMES F. W. JOHNSTON, A. M. &c. &c. Communicated by the Author.

SINCE the admirable experiments of Messrs Dulong and Petit published in the *An. de Chim.* for 1819, it has been rendered extremely probable "that the atoms of all *simple* bodies have

the same capacity for heat;" and, consequently, that the product of the atomic weight into the specific heat is in all simple substances a constant quantity; or $SW = C$, where S denotes the specific heat, W the atomic weight, and C the constant.

In the preceding number, (No. ix.) of this *Journal*, page 75, is inserted a paper by Mr Potter, in which he professes to give "an exposition of some erroneous determinations of MM. Dulong and Petit, and an examination as to the metals," of the above important law. On this paper I shall take the liberty of making a few observations.

Mr Potter endeavoured to determine the specific heats of eight different metals, by the old method of mixing known weights of the metals and of water of different temperatures, and marking the resulting temperature of the mixture. His results agree *essentially* with those of Dulong and Petit, except in regard to the three metals, silver, gold, and bismuth, in which the differences are as follow:

	Potter.	Dulong & Petit.	Difference.
Specific heat of Silver,	= .063	.0557	+ .0073
Gold,	= .046	.0298	+ .0162
Bismuth,	= .039	.0288	+ .0102

These differences form the sum and substance of Mr Potter's paper. They are all in excess, and are the more deserving of examination, as they represent the specific heats of gold and bismuth as nearly one-half greater than they have hitherto been esteemed.

The old method adopted by Mr Potter is the easiest, but it is liable to many sources of error, some of which are ably pointed out by Biot in the fourth volume of his *Traité de Physique*. To escape from these sources of error, Messrs Lavoisier and Laplace invented their calorimeter, and Messrs Dulong and Petit adopted the expedient of employing a small cylinder of silver filled with the metal to be experimented upon in the state of powder. All have agreed in considering the method of mixtures as incapable of yielding any precise results, and therefore we are led, *a priori*, to regard these high determinations of Mr Potter with some degree of distrust.

This distrust is increased when we inspect the results obtained by Mr Potter by inverse methods. These differences for

gold and silver, two of the metals for which he has given new, and, as he considers, more correct results, are as follow :

	1st Exp.	2d Exp.	Difference.
Specific heat of silver, =	.0601	.0669	.0068
gold, =	.0375	.0509	.0134

On comparing these differences with those given above, we find that Mr Potter's two results differ nearly as much from one another as the mean of them differs from the results of MM. Dulong and Petit. Mr Potter adopts the mean of his two experiments, supposing, fairly enough, that the methods being inverse, the errors are likely to have opposite signs.

But, in the materials employed by Mr Potter, we shall find, I conceive, ample cause for rejecting, without farther hesitation, the specific heat assigned by him for these two metals. "For *gold, silver, and copper,*" he says, "I employed British coins;" that is to say, his gold was 22 carats fine, or an alloy containing $\frac{1}{12}$ th of silver or copper; and his silver contained 7.5 per cent. of copper,—the quantities, I believe, usually found in British coin. With these he attempted to determine the specific heats of *pure* gold and silver; and because the result he obtained differs from that of MM. Dulong and Petit, is it right in him to denounce theirs as erroneous?

Rejecting these two alloys, then, there remains only the specific heat of bismuth to be considered. Mr Potter's two experiments on this metal agree closely with each other; the two results being .0390, and .0393, while that of Dulong and Petit, as above stated, is .0288. It will be necessary, therefore, to advert to the concluding portion of Mr Potter's paper, before we can determine absolutely which of them is to be preferred.

It was the numerous mistakes contained in this latter part of the paper, together with the tone improperly as well as unjustly assumed towards the French philosophers, that suggested to me the propriety of the present remarks. Admiring, as all must do, the profound reasoning and exceeding ingenuity of Mr Dalton, in his *New System of Chemical Philosophy*, Mr Potter has entered into his views not only regarding heat, but also regarding the atomic weights of bodies, for which his book cannot now be considered as any authority. Had Mr Potter's chemical reading been a little more extensive, he would neither

have spoken of others so disparagingly, nor committed himself so broadly.

Mr Potter thus commences his observations on the law expressed above by the formula $S W = C$.

“ However, the law as enunciated by the French philosophers, that the atoms of all simple bodies have the same capacity for heat, when applied to the class of metals, *holds I find with singular fidelity in common temperatures, excepting in one very remarkable case, that of silver.* Whilst the exception remains, the law cannot be said to be established, much less can MM. Dulong and Petit in the least be said to have proved their proposition, as they have taken for the atomic weights numbers *completely arbitrary, without referring either to the opinions of chemists, or the results of analyses.* Whence they have obtained their numbers I shall now show. They appear to have appropriated such portions of Berzelius's numbers as suited their experiments. Thus, for bismuth they have taken three-fourths of this number, for lead one-half, for gold one-half, for tin one-half, for silver one-fourth, for zinc one-half, for tellurium one-half, for copper one-half, for nickel one-half, for iron one-half, and for cobalt one-third of this number, &c. &c.”

Now, after this sweeping condemnation of *their* mode of proving the law, and of the atomic weights they employed, let us compare their numbers with those of Dalton adopted by Mr Potter, and with those given by Dr Thomson in his lately published *History of Chemistry*.

	Dalton.	Dulong & Petit.	Thomson
Atomic weight of iron,	25	27.136	28
zinc,	29	32.24	34
copper, 56 or 28		31.656	32
silver,	90	54	110
tin,	52	58.8	58
gold,	60	99.44	100
bismuth,	62	106.4	72
lead,	90	103.6	104

A single glance at these three columns will show whether the numbers employed by Mr Potter or by the French philosophers in verifying the law of equal specific heats, are the nearer to the

weights generally received in this country ; and yet Mr Potter has the advantage of all the researches made by chemists during the thirteen years which have elapsed since the French memoir was published. There is not indeed the slightest ground for asserting that the numbers in column second are “ *completely arbitrary, referring neither to the opinions of chemists nor to the results of analyses.*”

Nor is this all. Mr Potter professes to compare the numbers of Berzelius with those of Dulong and Petit, in a table given in page 82 ; but finding them in the book he has consulted adapted to the oxygen scale, he multiplies them by seven to bring them to the hydrogen scale (!) thus obtaining and setting down a list of numbers which no one ever dreamt of as representing the atomic weights of these bodies. Though Dalton adopts the ratio of 7 : 1 as expressive of the relation of the atomic weight of oxygen to that of hydrogen, he ought to have known that Berzelius, whose numbers he thus multiplies, does not, and this would have prevented him from falling into such an error. Let me request his attention to the following extract from Dr Thomson's *History of Chemistry*, vol. ii. page 297. “ He (Dalton in the 3d volume of his System, published in 1827,) still adheres to the ratio 1 : 7 as the correct difference between the weights of the atoms of hydrogen and oxygen. This shows very clearly that he has not attended to the new facts which have been brought forward on the subject. No person who has attended to the experiments made on the specific gravity of these two gases during the last twelve years, could admit that these specific gravities are to each other as 1 : 14. If 1 : 16 be not the exact ratio, it will surely be admitted on all hands, that it is infinitely near it.”—“ Dr Ure,” says Mr Potter, “ adopts 7.5 as an atom of oxygen.” In which of the ancient authors does Mr Potter find this?

But in the paragraph already quoted from Mr Potter's paper, while he denies that *they* have proved the law of equal specific heats, he says the law “ *holds I find with singular fidelity in common temperatures, excepting in one very remarkable case, that of silver.*” Now let us take the atomic weights of Dr Thomson, and multiplying them separately into the specific

heats obtained respectively by MM. Dulong and Petit, and by Mr Potter, let us see how the constant C comes out in each.

	Specific heat or S.		Atomic weights or W.	Constant or C.	
	Dulong & Petit.	Potter.		Dulong & Petit.	Potter.
Iron,	.110	.110	28	3.08	3.08
Zinc,	.0927	.098	34	3.158	3.332
Copper,	.0949	.096	32	3.0368	3.072
Silver,	.0557	.063	55	3.0635	3.465
Tin,	.0514	.056	58	2.9812	3.248
Gold,	.0298	.046	100	2.9808	4.6
Bismuth,	.0288	.039	72	3.1104	2.808
			108		
Lead,	.0293	.032	104	3.0472	3.328

If the law in question be true, the value of C obtained from each metal must necessarily approach very nearly to the mean of all the values. Now the mean of column fourth is 3.057, and all the values of C in that column are so near this mean as to be completely within the limits of error, the greatest difference being that of zinc, which is + .101. The mean value of C in column fifth, deduced from Mr Potter's specific heats, is 3.366, but the differences are more irregular than in column fourth; and the greatest is that of gold, which is 1.233 above a third of the whole. In the one of these columns then, the value of C or SW is nearly constant, in the other it is very irregular; we are forced, therefore, to the conclusion, that *either the law of equal capacities does not hold, or Mr Potter's results are inaccurate.*

But in column fifth there are three numbers which are conspicuously beyond the mean of the other five, and which, therefore, in other cases would be rejected in endeavouring to deduce an accurate mean. These numbers stand opposite to the three metals silver, gold, and bismuth. The mean of the other five values is 3.196, which is so near the mean deduced from column fifth that we may safely rely upon the truth of the law to which we have so often adverted—though, so far from showing it “to hold with singular fidelity,” we have seen that were Mr Potter's results to be depended upon, the law, in the present state of our knowledge, would be wholly untenable.

Let us turn now to the specific heat of bismuth, from which

we have stepped aside so long. We have been obliged to reject from column fifth the numbers opposite to silver, gold, and bismuth; the presumption, therefore, is, that the specific heats assigned to these metals by Mr Potter are too great. But it has already been shown for other reasons, that those given for gold and silver are not at all to be depended upon. We are justified in concluding, therefore, that his result for bismuth is also to be rejected.

Opposite to bismuth, in column third, I have inserted two atomic weights, that of Dr Thomson 72, and that adopted by MM. Dulong and Petit, which is one-half more, 108; and while I have multiplied the specific heat of these latter gentlemen, as they did, by 108, I have given Mr Potter the benefit of the former number 72, as affording a value of C somewhat nearer the mean than the other number does. The atom of silver I have also set down at 55, as this halving of Thomson's number was necessary for both determinations.

Whether we adopt the one or the other of these numbers is entirely a matter of theory; combinations may yet be effected, such as to render it convenient to make similar alterations in regard to many of the metals. That bismuth may have an atomic weight of 13.5 or 108, is rendered more probable by some late experiments of Mr Phillips, by which he has made known several new compounds* of that metal, in which the ratio of 1 : 3 prevails among the atoms.

ART. XIV.—*A few Remarks on an Account of the "Meeting of the Cultivators of Natural Science and Medicine at Hamburgh in September 1830, by James F. W. Johnston, M. A. &c. &c."* as Communicated by the Author to the *Edinburgh Journal of Science*, New Series, No. viii. page 189. By Dr MUNCKE, Professor of Natural Philosophy at Heidelberg. Communicated in English by the Author.

THE account given by Mr Johnston of the Meeting of the Na-

* These are a *trinitrate* and a *trismuriate*. (*Ann. of Phil.* December 1830.) Dr Thomson had previously described and analyzed a *triscarbonate*, (*First Prin.* ii. p. 391,) and Berzelius a *tris-sulphate*. (*Gmelin's Handbuch*, i. p. 1009.) All these salts contain one atom acid to three of base. Were we to consider the atom of bismuth 108, then these would all be called *disalts*.

turalists and Physicians at Hamburgh, is, amongst all its interesting matters, blended with so many deformities, that it may not only be useful in general to correct them, but it must be even a satisfaction to Englishmen to be possessed of better information. Taking this account on the whole, it is a true and correct one ; but we are sorry to find it disfigured by palpable errors, arising from prejudice or misconception, or want of good information, or, the *tria juncta in uno*.

Before I proceed, I must beg to direct the attention of the reader to Mr Johnston's translation of *naturforscher* by *cultivators* ; this is erroneous. *Forschen* signifies to search, to seek, to inquire, to pry, to investigate, to examine, and, by dint of these combined efforts, to improve ; the German denomination consequently implies the progress of science by diligent inquiry, whereas the English adopted by Mr Johnston implies only or more of a stationary plodding. I hope it may not be considered superfluous thus to correct this false descriptive denomination of the naturalists, which might otherwise lead to a false conception of their object. I shall now proceed, having disposed of the head, to the body of the account.

I do not really mean to be harsh, (to make use of one of Mr Johnston's own phrases,) but cannot help remarking, that it must be considered a breach of hospitality to stigmatize the inhabitants of a town who have almost unanimously exerted themselves towards contributing to the most attentive and cordial reception of the assembled learned men, in the manner the narrator has done : " a luxurious people, of whom it may be truly said their god is their belly, (I suppose he meant to say their belly is their god,) &c. &c." Are we then to learn from a subject of the first commercial country in the world, that commercial industry and activity is to be treated with sarcasm and contempt ? According to my humble opinion, the mercantile profession is of full as much importance and as estimable as the learned ; and what progress could science and the arts have made without the aid of commerce and navigation, and without the capital procured by commercial industry ? Mr Johnston might have also learned, by application to proper channels and authorities, that Hamburgh, without being an university or seat of learning, may boast of having produced within its walls, men of European celebrity, as *Klopstock*, *Reimarus*, *Fabricius*,

Busch, Unzer, Gries, and the great composer *Bach*; and from its great estimation of learning, and the support given to developement of talent, is likely to have, from time to time, similar brilliant stars appearing on its commercial horizon. It may be equally unknown to Mr Johnston, that the readiness of the Hamburghers in promoting objects of science is of old standing; and of the general acknowledgment in later periods, Mr Johnston instances himself, *Benzenberg's Experiments on the Fall of Bodies*, (page 240.) But what Mr Johnston relates to have been done by the town for the assembly, is in such contradiction with many of his observations, that his best advocates could scarcely be able to protect him against the charge of inconsistency. With regard to the luxury of Hamburgh, Mr Johnston does wrong to judge by the splendid entertainments occasionally given to visitors; although it may be certain, taking a general view of it, that the Hamburghers, who are good calculators, have also in this instance not been out of their reckoning; for at the elegant and splendid breakfast at the botanic garden, it has been proved by evidence, that a great many of the learned men found also other points of attraction besides science; and as Mr Johnston called that hour "one of the happiest he spent in Hamburgh," we may take it for granted that he did not belong to the class of those who he describes (page 231) as having *wondered*.

Wealth is a natural and necessary condition of a great commercial city; but it must be acknowledged by the most prejudiced, that the inhabitants of that great emporium of German commerce understand to give it a most proper direction by philanthropic institutions, by developement of talent, and by a comfortable mode of living and liberal hospitality. In the family-circle of respectable merchants, Mr Johnston must, however, have found, if he has had the good fortune to frequent them, that nothing of ostentation and extravagance prevails there; but certainly a noble hospitality, and the visible desire to make themselves agreeable to their guests, without the latter being either merchants or rich; their respectable standing in the learned world being sufficient qualification to entitle them to deference and respect. (Having myself with many of my colleagues been in that predicament, I can vouch for the fact.) The conversation in such limited domestic circles displayed

more interest for scientific objects than I did expect ; and when the first modest restraint was broken, then I discovered a much greater portion of general knowledge amongst the men (and in several instances even among the ladies,) than might have been anticipated from mere mercantile and ordinary education. Not having been myself at Hamburgh before the last meeting, nor being connected by relationship or other ties to its inmates, I consequently could not have any claim of preference ; and I do trust I may be deemed an impartial observer on that account. Let it, however, be considered at the same time, that, if not all the members of the convention there assembled had the good luck to be introduced in society, it was owing to the nature and character of such a large meeting, of which all the members present could not possibly be of a distinguished or known reputation, otherwise the number of first-rate literati of Germany would be enormous, as those congregated at Hamburgh did not constitute the tenth part of the whole.

In private societies, as well as generally, it was not difficult to observe that the better class were not ignorant of the scientific purport of the meetings of naturalists, which they had even been long acquainted with from former meetings in other towns of Germany, and of course the invitation on the part of the senate and the citizens, could otherwise not have taken place ; and the many absurd observations Mr Johnston reports from hearsay (v. 208-210,) are, generally speaking, void of foundation, and as unbecoming and unfit to be introduced in a report of that description, as it would be credulous to give credit to them. It is possible that Mr Johnston or his faithful reporter may have heard something similar from some *fat* *, or idler ; but

* I hope I may be excused to make use of a French term here, which, however, in this instance, is the most expressive for the sort of character I meant to describe, and of which, as cannot be unknown to Mr Johnston, many are to be found at all times of the day in and about the Alster Pavillion, and even out of change-hours at the Boersen-Halle, (not Boursen-Halle, as Mr Johnston, (probably also from false reports,) repeatedly calls that establishment,) sipping their coffee, and puffing their segars, and ready to give and receive all sort of scandal, small talk, and anecdote. Such places of resort are most convenient to isolated strangers, but their stationary inmates very bad channels of information, even worse than *valôts de place*, who are at least useful to persons who are unacquainted with the locality of a town, and strangers to the language. An *honest valôt de place* would have told Mr Johnston that the entrance-money to Mr Bauer's garden, since its ad-

idlers are weeds of every soil and climate, and no large town is without them; but their stupid opinions can only mislead the uninformed. I must, however, be permitted to state my doubts, founded upon my correct information, that certainly some of the *inferior rank* of the inhabitants first complained that the city was to be burdened with the charge of board and lodgings of the visitors, but were afterwards not only well satisfied, but glad when it came to their knowledge that the strangers were come to spend their own money. People of that class are of the same illiberal spirit and stamp almost in every country, even Great Britain not excepted; but Mr Johnston and his respected countrymen would have just cause to complain, if the German would take from such a narrow-minded class of the nation, the standard for the national character. This is not the way to judge of the people, nor is it the means of promoting the closer connection of respective nations, so desirable and necessary for the promulgation of the sciences.

The office of president, and more so that of secretary at the meeting of naturalists, are so laborious and difficult, that it is much to be feared the association will find it no easy task in future to obtain proper men ready to occupy these intricate offices, but particularly the latter, should,—as has been the case in Hamburgh,—many of the congregated members exact to see their often but too contradictory and unreasonable pretensions attended to; it cannot, therefore, be read without some indignation “*that a very unfortunate and unsatisfactory choice had been made in the person of Dr Fricke* ;” after which observation it is ridiculous to see the sweetening of the dose added, that “*it is not my intention to say any thing harsh of Dr Fricke?*—” but this is, in point of consistency, on a par with many other parts of Mr Johnston’s account. Dr Fricke had been known to most of the members, having attended the preceding meeting at Heidelberg, and, from having been an active member himself on that occasion, made himself acquainted with the routine of the business and the arrangement of the proceedings; added to which he enjoyed the confidence of his fellow-inhabitants and the senate of Hamburgh, which, as well as his indefatigable activity, made him a most fit person for

missions to the same have been appropriated to the poor of Altona and Hamburgh.

this arduous and, as it proved, ungrateful office. Had Dr Lehman (in every respect a most respectable, unassuming, and modest man certainly,) been considered a more fit secretary than Dr Fricke, the latter would not have been chosen ; and here I must be permitted to place the vote of a whole meeting against an individual opinion in support of my argument.

Mr Johnston introduces in his account a great deal of irrelevant matter of too minor importance to fill many pages, and his readers would be very badly informed, indeed, were they to learn from it the manners and customs of the Swedes, the Danes, the French, and the Germans. Nothing is easier than ridiculing certain peculiarities of a nation or nations ; but is there one without it ? The philosopher, however, the liberal minded, and the man of good breeding, will respect in others what he claims for his own. This is a principle grounded not merely upon a proper moral feeling but on sacred precept.

Page 233.—Mr Johnston says, “ he has heard some stories when sitting tête-a-tête with a German naturalist ;” and in his innocent candour adding, “ that it is chiefly the *young men* who are indiscreet enough to tell them,” he commits the very same indiscretion of a *young GERMAN naturalist*, not tête-a-tête with a confidential reader, but before the great public—not over a glass of punch in the pavillion of the Alster, but in Dr Brewster's excellent *Journal of Science* ! But let the same public, and by the same respectable channel, be informed that the stories Mr Johnston did introduce were on the continent not even considered sufficiently well founded to be received in the ordinary cursory pamphlets and breakfast journals, as food for idle readers. I allude to the story of the king of Denmark and Professor Pfaff of Kiel, and the dialogue of a Professor and the Director of the Police at Vienna.—(*Page 235.*)

I know very well what gave rise to both these stories, the first being from the year 1820, and consequently quite out of date ;—the second happened in the year 1828 ; but having at that period been myself in Vienna, and having had the best opportunity of obtaining good information, I can positively assert that the story, as reported by Mr Johnston, is quite disfigured ; besides, every person of sufficient experience must be aware that reports of what has passed in the closet between monarchs and ambassadors ought to be received with caution.

What regards “the fear of arbitrary power,” I may be permitted to observe, that the republic of letters does not know of any such *fear*; and I do not hesitate to say, that learned men are generally and *highly* esteemed, as they show themselves zealous in the performance of their duty—the cultivation of the extensive field of science. If they deviate from that path, wandering into politics, opposing established order, and become factious partizans, then it is no more science which has to complain or to fear arbitrary power. That the meetings were not before frequented by scientific men from the Austrian dominions, with the exception of Count Sternberg, was merely to be attributed to the great distance from the former places of meeting, and the prejudice prevailing at their origin, that the advantageous results were not adequate to the loss of time and expence. It was not till the king of Saxony had received the assembly of naturalists in his capital, whose curiosities were most liberally and readily laid open to them, and afterwards at Berlin, when Alexander de Humboldt did not refuse the office of president, that the meetings were visited by the most distinguished learned men, and from that period their established scientific character may be dated. The association then attracted the attention of the members of the imperial family of Austria, who are not only distinguished as patrons of the arts and sciences, but many of them also by profundity of learning, and H. I. Majesty’s permission, or rather the invitation to fix the next meeting at Vienna, might be acknowledged as a sincere estimation of science. Should Mr Johnston, or those who know that beautiful metropolis only from stories of young men, happen to join the next meeting, they will then find that it exceeds in treasures for natural history many other towns, and is not behind any in point of liberality and hospitality towards strangers.

It is much to be regretted that Mr Johnston, who is not void of acuteness of observation, had not been more cautious in collecting his information, which ought to be the first duty of a faithful reporter, as it is also his best policy, in as much as he exposes the credit of the correct part of his account by the hearsay and deformed information, picked up without discernment, and introduced without discretion.

I cannot close these remarks without uttering my earnest wish, that the meetings of the Naturalists and Physicians of

Germany may continue to attract the attention, and to enjoy the benefit of knowledge, of the learned men of other nations, and that science may cement the union which commercial intercourse had originated, and paved the way for amongst nations. May the language of the Germans also become more and more familiar to foreigners, to enable them to obtain correct and unprejudiced information themselves, as well in a social as in a scientific point of view.

HEIDELBERG, *June 1831.*

ART. XV.—*Observations on Professor MUNCKE's Remarks on Mr JOHNSTON's Account of the Meeting at Hamburgh.*
In a Letter to the EDITOR from Mr JOHNSTON.

MY DEAR SIR,

I HAVE to thank you for your kindness in allowing me to peruse Professor Muncke's remarks on my Account of the Meeting of the German Naturalists at Hamburgh, before they were sent to press. Permit me to add a very few notes by way of reply.

Professor Muncke is a person of some note, an amiable man, I believe, and one that I was prepared to esteem. I had not the pleasure of forming his acquaintance at Hamburgh, for a mere introduction, followed by the occasional interchange of a sentence during the several days of meeting, cannot be called such; but I shall not soon forget his animated manner when in his travelling dress he entered the Stadthaus on the morning of the first day of meeting, and found himself among so many assembled Naturforscher. I at once set him down as an enthusiastic man, and in such men I have generally found something worthy of esteem and admiration.

It is therefore gratifying to me to learn from Professor Muncke that my account, "on the whole, is a true and correct one," even when he takes up his pen deliberately to point out my errors. I have gone wrong, he says, from one or all of three causes, from *prejudice*, from *misconception*, or from *want of good information*. From wilful error or misrepresentation he acquits me therefore, and so far he does me only justice.

Whoever has honoured me by the perusal of my account of the meeting at Hamburgh, will bear me out in saying, that it

is not written in a critical or censorious spirit. I went to that meeting with the hope of being gratified,—I was gratified,—and I have expressed my gratification. And yet, Sir, were I to sit down to find fault and to pick out blemishes in my own paper, I could produce a much more formidable list than Dr Muncke has done; for where so many topics are touched upon, while all is true and correct, on the whole, it is hardly possible to avoid some slight mistakes. Let us see how many of these Dr Muncke has detected.

First, then, I have translated *Naturforscher* erroneously. It is a “false descriptive denomination,” it seems, to call German philosophers *Cultivators of Science*. Cultivators, says the Doctor, and they know English well at Heidelberg, are mere *stationary plodders*,—and your Germans are no plodders! Dr Muncke is a Professor of Physics, and it does not become me to argue with him on the doctrines of mechanics; but it would be interesting to learn, in what part of his *Compendium der Naturlehre* he treats of *stationary plodding*. It must be intimately connected, I should think, with that interesting species of progression known by the name of the *Stand-still motion*.*

After this serious exordium, Sir, it was a great relief to find in the second paragraph, that the Doctor did not “*really* mean to be harsh with me,” and I am quite as grateful as need be for his forbearance. One of the main objects of his communication is to defend the Hamburgers from my assertion that they are a luxurious people. But how will his wrath be excited and his forbearance regretted by him, when I add further, that of all the cities I have visited, Hamburg is the one in which the tone of morality is the lowest,—in which the means of licentiousness are the most abundant, most accessible, and most luxuriously and publicly provided,—and in which the practice of immorality is least regarded. And does the learned Professor really think he can persuade his readers, that the Hamburgers either know or care any thing about science. It cannot be that in a city which nightly confines within its walls 120,000 souls, there should not be some scattered gleams of genius at times bursting

* “How many kinds of motion are there,” said a Glaswegian professor of physics to one of his *very* bright pupils. “Three, Sir,” was the reply. “Three! name them.”—“The Retrograde, the Progressive, and the *Stand-still motion*.”

forth, or that science even there should not have its rare yet faithful votary,—and I have not been backward in giving her credit for now possessing such men,—yet, even with these exceptions, the people are not the less a mercantile people,—heedless of science,—and in whom, however ludicrous total ignorance of scientific matters may occasionally have appeared, yet ignorance is in a great measure excusable.

The Doctor is a matter of fact man, and cannot tolerate the many *irrelevant* and *absurd* observations I have introduced; especially about the Hamburgers and their fear of being burdened by the arrival of so many strangers; and to set me right, he gives the result of his *correct*, which is in sum and substance just the same as my *incorrect* statement. I have introduced a few sentences of colloquy which the learned Professor did not hear, and therefore, forsooth, they are incorrect. One must not, it seems, enliven a dry account of a meeting of naturalists with a few *on dits*, but out steps one of your German professors,—exclaims against their absurdity, and wonders you should give credit to them. Poor calumniated Hamburgers! I shall never be able to face you more.

Now, regarding our opportunities for information, let me compare notes with the Doctor. By his own confession he never was in Hamburg before, and he has no connections through whom he could have obtained information regarding the city or what was going on. He talks, indeed, of a *valet de place* having been very useful in telling him about Bauer's garden and various other matters; but that is what the stranger who visits Hamburg for the first time cannot do without. He arrived, I believe, on the first day of the meeting, and he spent about eight days in all, during which time he was occupied, as all were, with the proper business of the assemblage. I had visited Hamburg previously; I spent nearly a month in it at the time of the meeting, and I have visited it since. My opportunities of acquiring information, therefore, both concerning the place itself, and what passed about the time of the meeting, I am entitled to say were superior to those of the Professor.—Alas! he will exclaim, that you should have made so little use of them!

But the Doctor cannot read without indignation “that a very unfortunate and unsatisfactory choice was made in the person

of Dr Fricke," who had been named secretary to the meeting. I mixed much among all classes of the *Naturforscher* while at Hamburgh, and made more new acquaintances perhaps than any other stranger at the meeting; and my persuasion is, that the choice gave general dissatisfaction, and was therefore unfortunate. He must have been particularly kind to Professor Muncke to have gained so far upon his good graces. And if the selection was so generally acceptable, what may Dr Muncke mean when he says, (p. 275,) "the association will find it no easy task in future to obtain proper men ready to occupy the intricate office of secretary, should,—*as has been the case in Hamburgh, —many of the congregated* members exact to see their often but too contradictory and unreasonable pretensions attended to." Of course it cannot mean any thing inconsistent with what he has elsewhere said.

I am particularly grateful for the correction in the spelling of the word Börsen. On referring to my note book, I find it is not an error of the press but a defect of knowledge in myself. It shows how diffusive is the Doctor's philanthropy when he has been at the trouble to write all the way from Heidelberg to inform me of my error.

There is little else to which I need advert. Dr Muncke is offended at the two anecdotes I have inserted in illustration of the arbitrary spirit of the German governments. He says they were not considered sufficiently *well founded* to be introduced into the breakfast journals of Germany. And yet he tells us that the two circumstances actually took place, the one in 1820, the other in 1828, when he was himself in Vienna! Does he really wish us to believe that every thing *well founded* finds its way either into the breakfast or dinner journals in Germany. The very exclusion of such anecdotes, which spread from north to south nevertheless, is just a proof of what I stated, that the governments are suspicious and arbitrary. Did Professor Muncke of Heidelberg, in the course of his travels, ever hear of such a thing as a censorship of the press? We have not now to learn, however plausibly he may talk the matter over to us, that in his fatherland the *republic* of letters is a mere name. And then as to the *foundation* of these two anecdotes, the one I had, among other persons, from a di-

plomatist, and from a Privy Councillor of his Danish Majesty ; for the other, Dr Muncke is sufficient authority. They may not, they cannot, be given as they occurred ; but the spirit I think is preserved, and that alone is important.

The Doctor waxes witty in page 276 at the expence of my discretion in telling these *on dits*. This is all fair and allowable in men so much older and more experienced than ourselves. But I would humbly suggest to him as a metaphysician, if it is not the knowledge of evil consequences to follow which constitutes the indiscretion in such cases,—and that, as we fear none in this free country, there can be no indiscretion even in publishing what men dare hardly whisper in Germany.

In conclusion, Sir, I have to regret that Professor Muncke, who, I am willing to believe, writes more conclusively on Natural Philosophy, should have said so much and so sweepingly of my incorrectness, and yet should have adduced so few, and these so trifling, proofs of his assertion. Had he made any grave charge, I should have met it in a corresponding style. Such as he has made, he will excuse me for treating lightly. I hope his next communication will be such as to do honour to your *Journal*, to himself, and to the university he adorns.—I am, Dear Sir, your's very sincerely.

JAMES F. W. JOHNSTON.

PORTOBELLO, 23d August 1831.

ART. XVI.—*Round Sterns*. By a CORRESPONDENT.

IN some former numbers of this *Journal*, in the *Edinburgh Philosophical Journal*, and also in the *Edinburgh Encyclopedia*, the important question of round sterns has with much generality been discussed ; and it is therefore with no ordinary pleasure that we find a splendid prize has been offered by the French Minister of Marine, for determining among all the forms that can be presented that particular one, which shall unite in the highest degree all the requisite conditions that the seaman, the naval architect, and the geometrician may require.

Our readers are aware that a vigorous contest was for a long time kept up respecting the *principle* of the round sterns ; but

time, which softens differences, and with a gentle hand moulds even prejudice itself into the form of truth, has in this case obtained another victory; and a figure which our gallant seamen had connected with the glorious recollections of Trafalgar and the Nile, is now,—with wisdom which cannot be too highly praised,—nearly if not altogether abandoned. The square stern, with all its massy and cumbrous forms, has indeed given way to another, more in unison with the great march of improvement now going on.

But while the principle of what is commonly called the round stern has with few exceptions been admitted in all its fullness, its best possible form has not been determined; and it would seem as if fancy, rather than the sound discretion which geometry imparts, had presided over the designs hitherto submitted to the world.

Among the infinite variety of forms which may be denominated “round,” there must be one which shall unite in a higher degree than any other, all the best conditions of strength, convenience and defence;—which shall secure to the brave sailor the greatest degree of comfort, add a new arm to his power in the day of battle, and secure to that portion of the frame-work of the vessel, the same admirable strength as distinguishes its other parts. It is this choice of forms, which the French minister of marine now invites the naval architect, the sailor, and the man of science to contemplate; and we hail the call as a revival of the days when the great men of the Academy of Sciences, clad in the armour of the transcendental geometry, descended from the lofty elevation of the system of the world, to contend for the conditions of the metacentre, the great principles of stowage, the problems of masting, of pitching and of rolling, and all the other complicated but interesting inquiries, which the general question of ship-building involves.

Our *Journal*, read alike by geometricians, the naval architects and the sailor, can hardly be better employed than in recording the conditions which the French minister has proposed for this great problem.

“To furnish the best plans for the circular sterns for line of battle ships and frigates, with all the exterior and interior fittings, the manner of disposing the timbering so as to combine the necessary conditions for defence, with strength, lightness, a

dispersion of the weight in proper proportion to the displacement of each part, the efficiency of the rudder, the convenience of the water-closets, and the general suitableness of the accommodations.

“ This manner of fitting the stern must possess facilities for enabling the commandant to be aware of whatever manœuvres may be in progress, without being obliged to appear on deck.

“ The style of ornament which it would be proper to adopt, as well for the forward as for the after part of these new constructions, is also to be described. The competitors are to remember that nothing of importance is to be at all sacrificed to these decorations.

“ The side of the ship at the stern must have the same thickness as at the corresponding places in other parts of the ship. The ports must be so disposed, that it may be easy, on each deck, to bring guns to bear right aft and on the angles of the quarters, to command those points which the other guns cannot be brought to bear upon.

“ The rudder may be fitted either without board, or within with a circular head, but reasons must be given for whatever plan may be proposed. Reasons also are to be stated for the station which may be proposed for the water-closets, whether they are fitted interiorly, or in an exterior gallery.

“ The officers of the different branches of the naval service are called upon to send their proposals to the minister before the 1st of July 1832. *Other persons wishing to become competitors, are eligible to do so.* *

“ The memoir in which each competitor explains his proposals, must be accompanied with all the calculations and drawings which may be necessary to render his plan perfectly complete and intelligible in all its details.

“ Each proposal must have a motto affixed to it, of which a copy is to be enclosed in a sealed letter, containing also the name and place of residence of the proposer.

“ A medal of the value of 2000 francs will be given to the author of the best memoir presented to the minister of marine before the stated period.”

* The field is therefore open to our countrymen.—ED.

ART. XVII.—*Observations on the Theories which have been proposed to explain the Vitriified Forts of Scotland.* By S. HIBBERT, M. D. F. R. S. Ed. &c. &c. (From the *Archæologia Scotica*, vol. iv.)

THE members of this Society are no doubt familiar with what is meant by a Vitriified Fort. By this term is implied an area of ground, often of a round or elliptical form, and evidently selected for some natural defence possessed by it, which is farther protected by one or more inclosing ramparts, formed by stones; these stones showing, to a greater or less extent, marks of vitrification, by which they are cemented together.

None of these vitriified forts exhibit, as from many writers we should be erroneously led to suppose, any regular masonry in their structure. Unhewn fragments of stones, and water-worn boulders, sometimes mingled with smaller gravel, appear in a quantity almost exceeding belief, following the contour of the summit of a mountain, or, as in the instance of a fort which is situated in the Kyles of Bute, following the contour of a small holm or islet, elevated a few yards only above the level of the sea; and in cases where, owing to the more exposed nature of the ground, a stronger defence is demanded, a double or even treble rampart of the same rude materials is added. The vitrification which characterizes these forts is, in some few of them, displayed to an extent that is perfectly astonishing; while in other instances it is with difficulty to be detected. In short, no two forts in their degree of vitrification are in any respect conformable to each other; and it is of importance to add, that throughout Scotland numerous forts appear, only differing from those which form the present object of inquiry in the absence of all marks of vitrification whatever.

These forts first met with scientific attention about half a century ago, when various theories were proposed to account for the origin of their vitrification. At these I shall glance in succession.

1st, *The notion that the vitrification observable in these forts was the result of volcanic agency.*

During the last century it was a very prevalent notion, that

the vitrification of these forts had a volcanic origin ; and hence that volcanoes at some remote period had been very common in Scotland. This opinion was embraced by Mr Pennant, the eminent naturalist, who was led to it from an examination of the Hill of Craig Phædrick. A similar view was taken up by Thomas West, Esq. the author of a paper published in the *Transactions of the Royal Society of London* for the year 1777; and, four years afterwards, by the Hon. Daines Barrington.

The circumstances which led to this opinion have not been ill explained by Dr James Anderson of Monkshill, Aberdeenshire, in a letter dated the 27th November 1777, and read to the Antiquarian Society of London “ It must be owned,” says this author, “ that the natural appearance of the places where these vitrified masses are usually found, is well calculated to favour the opinion that they have been produced by volcanoes. The vitrifiable matter is usually first discovered by travellers around the bottom and on the sides of steep hills, frequently of a conical shape, terminating in a narrow apex, exactly resembling the hills that have been formed by the eruptions of volcanoes. It is therefore very natural to think that these may have been produced the same way. Let us, for instance, suppose that a traveller strongly impressed with this idea, should resolve to examine the top of the mountain more nearly, and for this purpose ascend to the summit :—would not his former conjecture be much confirmed when, at the top, he should find himself in a circular hollow, surrounded on all sides by matter rising gradually higher to the very edge of the precipice, which is there entirely environed with vitrified matter of the same kind with that which he had found at the bottom ? Could such a man be called unreasonably credulous if he should be induced by so many concurring circumstances to believe that this had been a real volcano ? But would he not be reckoned sceptical in the extreme if he should entertain the smallest doubt of the truth of this opinion, upon seeing the very opening itself in the centre of the hollow, through which the boiling lava had been ejected ? ”

Such is the liberal excuse assigned by Dr Anderson for the volcanic hypothesis, which is the more generous, as he came prepared to advocate a very different theory. Indeed, from

my own experience, I do not hesitate to say, that some few of these forts are not ill calculated to favour the deception. In a hasty survey which I took of the hill of Finhaven (the first vitrified fort I examined) I saw, like Mr Pennant, an apparent crater-formed cavity, filled with fused materials, occurring in such an abundance as to render unsatisfactory any explanation which has been hitherto given of the artificial mode in which vitrification to this immense extent must have been produced. But my illusion was short-lived ;—in the examination of a second example it was instantly dispelled.

2dly, *The theory, that vitrification was artificially induced, as a cement for the consolidation of ramparts of loose stones.*

In the year 1777, the validity of the volcanic theory was first opposed in a pamphlet published by Mr John Williams, mineral surveyor and engineer for the forfeited estates of Scotland.

This author, in advocating the artificial origin of vitrified forts, first assured himself, on the authority of a late eminent chemist, that many varieties of the rocks of Scotland were with little difficulty fusible. Dr Black's letter to him on this subject may perhaps be worth quoting : “ There are in most parts of Scotland different kinds of stones which can, without much difficulty, be melted or softened by fire, to such a degree as to make them cohere together. Such is the greystone, called whinstone, which for some time past has been carried to London to pave the streets. Such also is the granite or moorstone, which is applied to the same use, and pieces of which are plainly visible in some specimens of those vitrified walls which I received from my friends. There are also many limestones, which, in consequence of their containing certain proportions of sand and clay, are very fusible ; and there is no doubt that sandstone and puddingstone, when they happen to contain certain properties of iron mixed with the sand and gravel of which they are composed, must have the same quality. A puddingstone composed of pieces of granite must necessarily have it. There is abundance of one or other of these kinds of stone in many parts of Scotland ; and, as the whole country was anciently a forest, and the greater part of it overgrown with wood, it is easy to

understand how thoſe who erected theſe works got the materials neceſſary for their purpoſe.”

After Mr Williams had thus got over the difficulty of artificial fuſion, he promulgated his view, which, as he ſtates with much naiveté in his preface addreſſed to Lord Kames, appeared ſo incredible that no booksellers of London would hazard the publication of it. He ſuppoſed that the ancient inhabitants of Scotland derived their earlieſt notion of vitrification, “ firſt, from melting bog ore, in the proceſs of which they learned how it would, if increased, vitrify ſtones and earth; ſecondly, from obſervations made while roaſting oxen. It is evident,” adds this choice antiquary, “ from the records of the earlieſt antiquity, that it was the practice of almoſt all nations to offer burnt ſacrifices. And it was cuſtomary, on ſolemn occaſions, to burn a large-sized animal whole, which would require a very ſtrong fire.”—“ After they [the ancient inhabitants of Scotland] became acquainted with this power of heat, I ſuppoſe ſome genius among them to have employed it for forming vitriſied walls. The manner, however, in which he employed it is ſtill problematical. I have tried the ſubject ſeveral ways in my own mind, but find difficulty in all the methods I have yet imagined. I ſhall, however, mention that which ſatisfies myſelf the beſt, and which appears moſt practicable. I imagine they raiſed two mounds of earth parallel, and in the direction of their intended wall; and that they filled the ground formed by theſe parallel mounds with fuel, above which they laid the materials to be vitrified. There is no doubt but a ſtrong fire would fuſe theſe ſtones, eſpecially if they were of the plum-pudding kind, and not too large; and the frame of earth would prevent them, when in fuſion, from exceeding any aſſigned breadth. I ſuppoſe they added layers of freſh fuel and materials alternately, and raiſed the mounds of earth till the whole was brought to the intended height. I am confident, from the appearance of the ruins, that the materials of them were run down by the fire in ſome ſuch method as this. For in all the ſections which I have ſeen, whether of the larger or of the ſmaller fragments of theſe ruins, I never obſerved the leaſt appearance of a ſtone being laid in any particular way.

Nor could I in any of them discover a stone, large or small, not affected by the fire, and in some measure vitrified."

In connection with this theory, Mr Williams defines the walls of a vitrified fort, when complete, after the following manner:—"The walls are vitrified or run and compacted together by the force of fire, and that so thoroughly, that most of the stones are melted down, and any part of them not quite run to glass is entirely enveloped in the vitreous matter; sometimes also they appear like vast masses of coarse glass or slag."

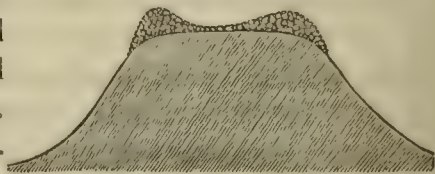
But we shall now examine, with some degree of attention, the validity of this theory, particularly as it has recently met with a defence from a writer of no small eminence, who possesses a store of antiquarian and mineralogical knowledge, which eminently qualifies him to enter the lists in the determination of this question. I allude to a memoir published in the *Transactions of the Geological Society of London*, by Dr Macculloch, and to his views on the subject which are contained in his description of the Highlands and Western Isles of Scotland.

The drift of the theory in question is,—that with the design of strengthening the walls of a fort, the stones have been cemented by a regular process of vitrification.

In support of this view Mr Williams has stated, that in none of the vitrified forts which he examined could he discover a stone, large or small, unaffected by the fire, or which was not in some measure vitrified. This assertion, if correct, would certainly go a great way to urge conviction, that the vitrification of these forts was not an accidental or adventitious, but a designed process. Never was there, however, a more unfounded statement. At Dun Evan, where there is an extensive fortified site, the vitrification so far from having acted upon every stone, is only to be detected in a space the dimensions of which do not exceed two or three yards. I can also avow, without dread of contradiction, that in numerous other sites the vitrification is only to be found in small patches, or that extensive portions of the ramparts which have composed their defence do not show in the slightest degree the effects of fire. Nay, even in the forts which display the greatest extent of vitrification, considerable intervals may be readily traced where no

fusion of the stony materials has ever taken place. These circumstances, then, are fatal to the notion that vitrification was the effect of design; they rather show that it was merely incidental to some other view, which the authors of it must have contemplated.

This theory is again as ill supported in its details. Mr Williams has supposed that the builders of these forts raised two mounds parallel to, or in the direction of their intended wall, and that they filled the ground formed by these parallel mounds with fuel, above which they laid the materials intended to be vitrified. Now this supposition is faulty, inasmuch as it proceeds upon an incorrect view which has been taken of the actual construction of these forts. A wall formed in such a manner would present to view a pile of stony materials, with sides which would be vertical, or at least nearly so; and it would inclose the summit of a hill after the manner of a modern park wall. But this is not the character of a vitrified fort. The structure of the least dilapidated example is much more rude, being rather after the model of a filled up or extinct volcanic crater. Upon the summit of an insulated hill, an incredible accumulation of loose stones has been made to rise to the greatest height around its circumference, and to gradually thin off towards the centre of the enclosed site. An original form of this kind could not then have been derived from any parallel mounds. This is evident from the annexed imaginary section.



These are the chief reasons which induce me to regard the theory of Williams as perfectly untenable.

3dly, *The theory of Dr Anderson, that vitrification was promoted by the employment of a peculiar vitrescible ore.*

The theory of Williams, during the same year in which it was divulged, met with some qualified support from Dr Anderson, in the dissertation by him to which I have already alluded. He observed of the walls of Knockfarril, that "they consisted of stones piled rudely upon one another, and firmly cemented together by a matter that had been vitrified by means of fire, which formed a kind of artificial rock that resisted

the vicissitudes of the weather better perhaps than any other artificial cement that had ever yet been discovered."

Thus, in coinciding with Williams, that vitrification was intended as a cement to strengthen the walls of forts, he only differed from him in the more correct view which he took of the real structure of these defences, or in the theoretical use which he made of an extraneous vitrescible matter. He properly remarked of the vitrified fort of Knockfarril, that its wall was continued quite round the area, being adapted to the form of the hill, so as to stand on the brink of a precipice, all round, with the exception of the places of entrance. Such being its construction, he next supposed that vitrification was effected by the aboriginal Britons after the following manner :

"Through all the northern parts of Scotland, a particular kind of earthy iron ore of a very vitrescible nature much abounds. This ore might have been accidentally mixed with some stones at a place where a great fire was kindled, and being fused by the heat, would cement the stones into one solid mass, and give the first hint of the uses to which it might be applied. A few experiments would satisfy them of the possibility of executing at large what had been accidentally discovered in miniature. This knowledge being thus attained, nothing seems to be more simple and natural than its application to the formation of the walls of their fortified places. Having made choice of a proper place for their fort, they would raise a wall all around the area, building the outside of it as firm as they could of dry stones piled one above another, the interstices between them being filled full of this vitrescible iron ore ; and the whole supported by a banking of loose stones piled carelessly behind it. When the wall was thus far completed, with its facing all round reared to the height they wished for, nothing more was necessary to give it the entire finishing, but to kindle a fire all round it sufficiently intense to melt the vitrescible ore, and thus to cement the whole into one coherent mass, as far as the influence of that heat extended. As the country then abounded with wood, this purpose would be readily effected by building a stack of wood round the whole outside of the wall, and then setting it on fire. It was probably with a view to enable them to build this stack of wood

with the greater ease, and to suffer the fire to act more forcibly and equally upon the different parts of the wall as it gradually consumed, that they were induced to incline the walls so far from a perpendicular position. In an after period, when the woods had gradually been destroyed, and before it was well known how to manufacture peat for fuel, it would be such a difficult matter to procure fuel in abundance, that buildings of this kind would come to be disused, and the art in a short period, among a people ignorant of letters, be entirely forgotten."

Such is the explanation of the process of vitrifying, as given by Dr Anderson, with the discovery of which he is so delighted that he patriotically adds, "I am disposed to believe that this has been entirely a British invention, and think it probable that the art was never carried out of this country." He then laboured to prove that so admirable an art was unknown to the Danes, who endeavoured to vitrify some walls round the peninsulated rock of Broughhead; but having blundered by getting hold of some wrong vitrescible ore, the experiment failed.

On this view of Dr Anderson nothing more may be remarked, than that it is in contradiction with the real facts concerned in this vitrification. All later observers have agreed, that the fusible matter does not consist of any peculiar vitrescible iron ore found in Scotland, but that it is derived for the most part from the alkaline ingredients of well-known rocks of granite, gneiss, mica-slate, clay-slate, hornblende schist, sandstone, &c. &c.; these rocks being more or less fusible in proportion to the felspar which they contain.

4thly, *The theory of Lord Woodhouselee, that fire has not been employed in the construction, but towards the demolition of such forts as display the marks of vitrification.*

In a paper published in the year 1787, in the *Transactions of the Royal Society of Edinburgh*, Lord Woodhouselee successfully exposed the weakness of the theory first proposed by Williams, and supported to a certain extent by Anderson and other writers.

His Lordship proceeded upon the correct ground, that the vitrification of forts was a very partial occurrence; that three fortified summits which he examined were crowned with stone

structures, showing no appearance whatever of the effects of fire ; and that the number of such forts as displayed marks of vitrification would be found inconsiderable, when compared with those which have not been at all affected by heat.

The same able writer also questions, with much propriety, the capability of constructing an efficient fortress of vitrified materials. He doubts whether it would be at all possible, even at the present day, by the utmost combination of labour and skill, to surround a large space of ground with a double rampart of stones compacted by fire, of such height and solidity as to serve any purpose of security or defence against a besieging enemy ; and concludes that any structure of this kind must have been irregular, low, fragile, easily scaled, and quite insecure.

The hypothesis of Williams having been thus shown to be perfectly unsatisfactory, Lord Woodhouselee next proposed a theory of his own. It was suggested by the observations which he made, that vitrified mounds do not appear to have been ever much higher than they are at present ; that even in those sites where the wall was the least elevated, the fragments which have fallen down from it were very inconsiderable. His Lordship was therefore led to suppose, that the ramparts which now remain convey to us a full notion of the original construction of these forts, which was not of stones only, but which depended for their chief defence upon the wood which was employed in their fabric. He supposed that the building was begun by raising a double row of palisades, or strong stakes, in the form of the intended structure, in the same way as in that ancient mode of building, described by Palladio, under the name of *Riempiuta* or *à Cassa*, coffer-work ;—that these stakes were probably warped across by boughs of trees laid very closely together, so as to form two fences, running parallel to each other at the distance of some feet, and so close as to confine all the materials, of whatever size they might be, that were thrown in between them ;—and that into this intermediate space were cast boughs and trunks of trees, earth, and stones of all sizes, large or small, as fast as they could be quarried or collected ; very little care being necessary in the disposition of

these materials, as the outward fence would keep the mound in form.

The learned writer having thus constructed a fortress of his own, or rather one that was borrowed from an Italian architect, proceeded to develope his newer theory of vitrification, which was, that in a structure composed of combustile and fusible materials, the effects of fire would be accidental; that they would be induced, not during the construction, but during the demolition, of a fort.

“The most formidable engine of attack,” remarked his Lordship, “against a structure of this kind, would be fire; and this, no doubt, would be always attempted, and often successfully employed, by a besieging enemy;—and if the besiegers prevailed in gaining an approach to the ramparts, and, surrounding the external wall, set fire to it in several places, the conflagration must speedily have become general, and the effect is easy to be conceived. If there happened to be any wind at the time to increase the intensity of the heat, the stony parts would not fail to come into fusion, and, as the wood burnt away, sinking by their own weight into a solid mass, there would remain a wreck of vitrified matter tracking the spot where the ancient rampart had stood, irregular, and of unequal height, from the fortuitous and unequal distribution of the stony materials of which it had been composed.”

This theory, in its application to a country so well wooded in times of yore as Scotland, merits some notice, though it must be confessed that it is much easier to frame a notion of this kind, which is little more than the offspring of a fertile imagination, than to completely refute it. The chief objection which I have to this hypothesis is, that in many forts which I have examined there is no indication whatever that their structure was at any time different to what they display at the present day; and that the encompassing stone mounds appear to have afforded a sufficient defence to a savage people, without the addition of ramparts of wood. His Lordship must have anticipated the latter objection, by citing the exceptions to his theory, which he explains after the following manner:—“In those parts,” he observes, “where stones would be easily quarried of such size and form as to rear a rampart by themselves

of sufficient strength and solidity, there was no occasion to employ wood or turf in its construction, and it was therefore proof against all assault of fire. Such are the ramparts which appear on the hill of Dun Jardel, Dun Evan, &c. But on Craig Phædrick and other hills, where, from the nature of the rock, the stones would be procured only in irregular and generally small fragments, it was necessary to employ some such mode of construction described."

These are the chief reasons which have induced Lord Woodhouselee to form an opinion differing from that of Mr Williams, and of such as believed these structures to be the proofs of an ancient mode of building, in which fire was employed for the purpose of a cement before our ancestors knew the use of lime. He is rather disposed to conclude that the appearances of vitrification, which a few of these fortified sites evince, are the accidental effects of fire upon a structure composed of combustible and fusible materials, and by no means the consequence of an operation intended to produce that effect.

My own opinion of his Lordship's *demolishing* theory is scarcely a decided one. I cannot think it likely that in all cases structures of wood were called in aid to give additional security to mounds of stone. The probability, however, of rude wood-work being *occasionally* employed for this purpose must be admitted; and perhaps it may be illustrated in forts of very small dimensions. But that vitrification was induced by their being set on fire by a besieging enemy, is a distinct proposition, which in most instances will, I am persuaded, meet with no countenance whatever.

5thly, *The opinion that the vitrification of these forts was the result of beacon-fires.*

A fifth and last opinion, said to be the most plausible one that has yet been broached, is, that the vitrification of these forts was the result of beacon-fires. This theory has met with many supporters, particularly among the contributors to Sir John Sinclair's *Statistical Account of Scotland*. But the most able advocate of this opinion is Sir George Mackenzie of Coul, Bart in an article on vitrified forts, written by him for Dr Brewster's *Encyclopædia*, and in his published letter addressed to Sir Walter Scott, on the vitrified fort of Knockfarril.

The chief arguments for this opinion are, that the marks of fire are indicative of an accidental rather than of an intentional effect, and that vitrified forts are generally situated on lofty insulated hills, in such a chain or mutual connection as to allow of telegraphic communications to be conveyed from one station to another at a considerable distance.

But even this theory is not without its difficulties. In a dissertation lately printed in a volume of the *Transactions of the Royal Society of Edinburgh*, by James Smith, Esq. of Jordan Hill, a fort in the Kyles of Bute is described, which occupies the site of an islet or holm not more than twelve or fifteen feet above high water mark. Arguing therefore from this circumstance, as well as from the regularity of its vitrification, the writer comes to the conclusion, that the effect of signal-fires will not account for the fort in question, because the situation, in a flat surrounded on every side by hills of considerable elevation, does not appear at all calculated for such a purpose ; and because, in the next place, the regularity of its form seems still more inconsistent with the effects of an accidental cause.

Many who have supposed that the vitrification of these ancient remains was the result of beacon-fires have offered conjectures on their date. In a communication read to the Philosophical Society of Manchester, by Dr Milligan, the author is of opinion that they were in use among the earliest inhabitants of Caledonia ; and he supposes that, as the invasion of Agricola was attended by a fleet on the coast of Scotland, the fires seen in the interior of the country, which Tacitus describes as the flames of dwellings kindled by the inhabitants, might have been signal-fires communicating from hill to hill, as, for instance, from Stonehaven to Bute, where a line of vitrified forts may be traced ; and that this telegraphic communication was the prelude of the battle of the Grampians. Various other writers, however, assign to these forts a much later date, particularly the contributors to Sir John Sinclair's *Statistical Reports*. They conceive that they were in chief requisition as beacons during the descents of the Northmen, which lasted several centuries.

This last opinion many, if not most, of the vitrified sites

which have been examined, tend greatly to support. The coasts of Scotland began to be annoyed by the predatory visits of the Vikingr about the end of the eighth century; but it was not probably until the Scots had obtained a complete ascendancy over the Picts, by which both were united under one government, that systems of beacons were formed to provide against the sudden descents of the Scandinavians, who invaded them from the Danish or Norwegian shores, or from countries which they subsequently colonized, namely, from Shetland, Orkney, Caithness, Sutherland, the Hebrides, Ireland, or the Isle of Man. The Murray Frith, as we learn from ancient Sagas, was one of the most convenient landing-places for the Northmen; and hence we must look to this locality for the greatest proportion of vitrified sites. The number of such as have been traced within sight of each other, in a direction east to west from Banff to Dingwall, and in a direction north to south from Cromarty to Fort Augustus, may be estimated at twenty; but it is probable that their actual amount will be eventually found to be much more. Two vitrified eminences appear near Huntly, connected apparently with the line of coast extending from Kinnaird's Head to the mouth of the Dee. More south, a chain of vitrified sites, nine or ten in number, appears to have conveyed signals from the line of coast which stretches from Kincardine to the Tay, being prolonged from Stonehaven or Dundee to the neighbourhood of Dunkeld or Crieff. On the west coast, again, we find similar vitrified sites at Bute, Cantire, Isla, Loch Etive, Loch Sunart, Fort William, or at Arisaig. The number which subsists on the west yet remains to be ascertained; about twelve have been enumerated. Lastly, at Galloway, three occur, apparently as signals against the marauding colonists of Ireland.

But it must be kept in view, that it is highly improbable that the original intent of the whole of these vitrified sites was that of affording convenient localities for beacon-fires. There is no necessity, whatever, *ceteris paribus*, that signal lights, merely giving notice of the approach of an enemy from the sea, should be situated on fortified stations. It is not improbable, therefore, that the rude ramparts themselves might, at a still more remote period, have been erected by the oldest in-

habitants of Albyn, namely, by the Caledonians and Picts; and hence their occasional or very partial vitrification would merely indicate the later use to which the eminences upon which they were situated had been applied, when the Scots, who succeeded to far earlier inhabitants, were called upon to repel new invaders from the sea.

Again, it may be remarked, that after the Northmen had gradually acquired a permanent settlement in Scotland and elsewhere, they themselves became liable to sudden piratical descents from their own coutrymen; to guard against which, ancient Sagas inform us, they instituted, in every country where their arms prevailed, still more perfect systems of beacon-fires, the wardenship of which was enforced by the most rigorous laws. Edicts on this subject appear in many early codes of the north of Europe, particularly in the *Leges Gula-Thingenses of King Magnus of Norway*, where, in the *Land-varnar Bólkr*, we find that the bonds were plighted during time of war to be prepared with watch-fires in places where the same had been lighted up from old time. (*Magnus Konongs Laga-Bæters Gula-things-Laug*. Havniæ, edition, A. D. 1817, page 85.) Martin also states of the ancient beacons of the isle of Harris, which the Norwegians colonized, that “there are several heaps of stones, commonly called karnes, on the tops of hills and rising grounds on the coast, upon which they used to burn heath as a signal of an approaching enemy. There was always a sentinel at each karne to observe the sea-coast; the steward of the isle made frequent rounds to take notice of the sentinels, and if he found any of them asleep, he stripped them of their clothes and deferred their personal punishments to the proprietor of the place.” Still more information on this subject, yet of a general kind, may be found in the “*Historia de Gentibus Septentrionalibus*,” by Olaus Magnus, whose narrative, “*De Ignibus Montanis Tempore Hostili*,” is accompanied with a rude drawing of mountain-fires.*

The explanation which Olaus Magnus gives of this drawing is as follows: “Ex hac imagine duo veniunt consideranda: quorum alterum in vertice montium, fumi scilicet congestis

* A fac simile of the early wood-cut of Olaus Magnus is given in the fourth volume of the *Archæologia Scotica*.

lignorum struibus ad classicos insultus accedentium inimicorum arcendos excitati: alterum verò in littoralibus angustiis, et scopulis, ne hostes insiliant, diligens equestrium custodia intuenda: qui montana inhabitant loca tempore hostili, veluti celerissimi speculatores, signa fumo faciunt: quibus visis, alii reliquos montes incolentes, itidem a longè positis ignita pyramide demonstrant, ut quilibet armatus juxta numerum principis, et patriæ lege decretum, ex campestribus locis pro littorum custodia sine mora descendat. Inter quos velites equestres celerissimi adsunt, ut hostibus portuum, vel riparum accessum præclusuri, sagittariis plebeix multitudinis locum stuant, ubi commodius excipiant, ac conficiant hostem, omnino aggredi contendentem: ut scilicet in vallibus, aut cavernis expectent, vel ad iniqua et ignota hostibus loca, quasi fugitivi declinent, ne fortè eos taliter insequentes educere valeant in agmina robustiora: quæ urgente necessitate, usque ad infinitam multitudinem augeri solent. Nec desunt exploratores quoque verum emissi, qui renuntiant, qua ex parte adhuc immincant hostes, ut his celerius, et copiosius occurrentes, vel consilio, vel virtute, vel insidiis, vel necessitate, vel desperatione, vel loci securitate nutantibus, ne dum victoriam adimant, sed et victos, ut imperata perficiant, militari lege constringant.” (*De Gentibus Septentrionalibus*, &c. edition, *Romæ*, 1555, p. 228.)

Such is the account of these ancient mountain-fires which I have collected from Olaus Magnus. By another writer, it has been related of the Norwegian Monarch, Haco the Good, that he caused large trees to be formed into piles, and to be so placed as to be visible from mountain to mountain, with the view that intelligence of a hostile invasion might in seven days travel from one end of his kingdom to the other. “ Ut in montibus excelsis ex ingentibus arboribus pyre ita struerentur (s. angari) ut ab una pyra ad alteram facilis et liber esset prospectus. Excitatus hoc pacto hostilis irruptionis nuntius, a prima in extremo regni ad meridiem angulo extructa pyra, ad remotissimum boream versus publicorum comitorum in Ha-logalandia locum septem dierum spatio volitasse fertur.” (*Snorre Haconar Goda*, cxxi. p. 146.) The quotation is to be

found in Mr Turner's *Anglo-Saxons*, vol. ii. p. 258, 4th edition.)

From this information of Snorro, we are entitled to expect that vitrified sites should be found on the mountain tops of Norwegian provinces. But I am not aware that Scandinavian antiquaries have yet pointed out their existence.

Independently, however, of this historical testimony, most of the vitrified forts which I have examined show internal evidence of their having been in use for such incidental purposes as beacon-signals. Where the stones which have received the full force of the fires appear of inconsiderable depth, a complete fusion of the part has taken place; but, in other examples, the fused matter has run among the stones in small streams. In almost every case vitrification appears in patches, the cementing process not being a continuous, but a very limited effect.

For the extent of fusion, we must look to the comparative degree of frequency with which a beacon-fort would become in requisition; while the part of the fort which would be fused must depend upon the direction of the wind at the time a beacon was lighted.

It is also not improbable, that such a portion of a beacon-hill would be fired, as might convey the earliest information of the direction in which an enemy was approaching, or of any other events suggesting effectual plans of resistance. Thus, at the hill of Cowdenknows, on the borders of Berwickshire, although its summit has been fortified, it is on the flank of this eminence, where little or no defence appears, but which commands the view of a considerable tract of country to the north and north-east, that a small cairn of vitrified stones is to be detected. In many other places, also, vitrification is rather to be observed on the unprotected side than upon the defended summit of a hill; which circumstance might lead us to suppose, that signals of alarm were often intended to be concealed from an invading enemy, with the design that a readier chance of success might be afforded to stratagems of repulsion or surprise. That this view is not altogether theoretical, may be collected from the very ancient armorial bearing worn by Macleod, Lord of Lewis, where, in a rude representation given by Sir

David Lyndsay, Lyon King at Arms, in 1542, fires are made to blaze from many diversified points of a beacon-hill. But the authority of Wallace, who wrote in the year 1700, is more direct to the point. He has stated, that even at this late period "the people (of Orkney) had in every isle a wart-hill or ward-hill, which is the most conspicuous and elevated part of the isle, on which, in time of war, they keep ward; and when they see the enemies ships approaching, they put a fire, thereby to give notice to the adjacent isles of the nearness of the enemy, and to advertise them to be on their guard, or to come to their help; this they distinguish by the *number* of fires.

6thly, *Are there any other very ancient observances or customs, not hitherto cited by authors, which may have contributed to this vitrification?*

As it remains yet to be proved that marks of vitrification are uniformly limited to fortified sites, or even to beacon-hills, a natural question arises, "Are there any other ancient observances or customs which might have contributed to this vitrification?"

For such information we must consult the earliest and most obscure records of Scottish history. One of these which may be noticed, is the public festival said to have followed the labours of the chase, in the commemoration of which all the early Scottish traditions and songs agree. That this feast was accompanied by a sacrifice of fuel derived from the ancient forests of Scotland, to an amount which at the present day is almost incredible, might be inferred from the testimony of Ossian, if we could only depend upon the correctness of the translation: "Night is around the hero and his thousands spread upon the heath; a hundred oaks burn in the midst; the feast of shells is smoking wide." But upon this cause of vitrification it would be imprudent to insist, as I should be disposed to attribute to it very few effects in comparison with that of beacon-fires. I shall merely mention, that in a section which was made by Sir George Mackenzie of the very small vitrified fort of Dun Fian, near Inverness, the quantity of burnt bones of animals, and of charcoal, found on the site, would rather indicate the sequel of a feast than the effect of signal-fires. But

perhaps a combination of events might have occurred to produce this appearance.

Nor is it quite impossible, that some few occasional instances of vitrification in particular sites might have been rather indicative of the forest-trees that blazed during the immense feasts which the Vikingr were accustomed to enjoy upon each occasion of a *strand slaughter*; that is, when they assembled near some strand or shore to slaughter the cattle which they had captured, for the purpose of transporting the salted carcases to their winter haunts.

Another ancient observance connected with the lighting up of immense fires, was the festival in honour of Baal, which was renewed upon each return of the summer solstice. This was the greatest feast of the ancient inhabitants of Scotland, which even long outlived the introduction of Christianity; no ceremony belonging to it being subsequently omitted, except the sacrificial effusion of human blood. Indeed, the custom of the Baaltein is perpetuated in some few districts to the present time; and at the parish of Galstone in Ayrshire, a similar pagan honour characterizes the feast of a Christian Saint, the ancient custom being still retained of kindling fires on all the neighbouring heights upon the evening before a celebrated fair is held in honour of Saint Peter.—(*Statistical Account of Scotland*, vol. ii. p. 82.)

In short, we have only to conceive of an annual hunter's feast, a Baaltein, or indeed a periodical bonfire upon any public occasion whatever, continued for many centuries in an accustomed site, and during a state of country in which it was deemed patriotic to reduce the luxuriance of extensive forests, and we have a cause perfectly adequate to explain many instances of vitrification, which, I expect, upon inquiry, will be found to be of far greater frequency throughout Scotland than has hitherto been suspected.

7thly, *The probability that many of the sites in which vitrified remains occur, were places of rendezvous for tribes or clans, upon all public occasions of peace or warfare.*

Should this supposition be admissible, it might possibly explain the circumstance of some vitrified sites boasting little or no defence.

In fact, places of rendezvous for tribes and clans upon any public occasion, alike incidental to a state of peace or warfare, must have been suggested by many circumstances, as, for instance, by the custom of holding justiciary courts upon them, or even by religious veneration; the first of which motives recommended a cairn in the holm of Dalmorton as the rendezvous of some Ayrshire tenants, (*Statistical Account of Scotland*, vol. iii. p. 594,) while superstition taught the Buchanans to gather in the island of Clareinnis in Loch Lomond. Keeping this circumstance in view, we need not be surprised that vitrification, which was as likely to result from festive or religious bonfires, as from beacon-signals, should appear in a holm or islet of the Kyles of Bute, not elevated many feet above the level of the sea, or upon some inconsiderable eminence of the easiest access to an assailing enemy, which is the distinguishing site of Dun Fian.

Most places of rendezvous, however, were suggested by defensive motives. Thus, the ancient war-cry of the Campbell's in allusion to their assembling place, was the famous mountain in Argyleshire of Bencruachan; while, among the borderers, that of the Logans was Lesterich Law. And, in the use which we would make of these analogies, we may cite the vitrified fort of Dunardile, near the fall of Fyres, where, according to the etymology given of the name by a statistical writer, *Dun* signifies a hill; *Ard*, high; and *Dyil*, Carnochs or followers of a tribe. (*Statistical Account of Scotland*, vol. xx. p. 38.) In short, the sites of many forts, vitrified or unvitrified, instruct us, that, while they were intended as places of rendezvous for tribes, their ramparts afforded no less a defence for cattle than for human inhabitants; and upon many of these Duns, or strengths, of inappreciable age, the fire of the Beltein, or the beacon of war, has been in turns the symbol for the gathering of ancient clans.

At the same time it must be remembered, that in some instances a clan has had one site for its rendezvous, and another for its beacon-hill. Thus, in comparatively modern times, the Seaforth's mustered at the Castle of Donan, while the signal fires were lighted on the summit of Tulloch-ard.*

* Remains of similar observances may be traced in England even so
NEW SERIES, VOL. V. NO. II. OCTOBER 1831. U

8thly, *The ancient denſely wooded ſtate of Scotland, of which the number of vitriſied ſites, and the occasional intensity and extent of their vitriſication, ſerve as indications.**

The forests of Scotland, from the fifth to the fourteenth century, far exceeded in abundance or magnitude thoſe of South Britain. Among the produce of them are enumerated the oak, the pine, (*pinus ſilveſtris* or Scots fir,) the birch, the hazel, the broad leaved or Wych elm, the roan tree, (or mountain aſh,) the common aſh, the yew, the alder, the trembling poplar, (*populus tremula*,) the bird cherry, (*prunus padus*,) and the ſaugh or ſallow.

Mr Fraser Tytler has collected from the *Rotuli Scotiæ* the following liſt of ancient forests:—

The forests of Spey, the forests of Alnete, Tarnaway, Awne, Kilblene, Langmorgan, Elgin, Forres, Lochindorb, and Inverneſs.

In Aberdeenshire, the forests of Kintore, Cardenache, Drum or Drome, Stocket, Killanal, Sanquhar, Tulloch, Gaſgow, Darrus, Collyn, and the new forest of Innerpeffer.

In Banff, the forest of Boyne.

In Perthſhire, a forest which, in David I.'s reign, occupied the lands between Scone and Cargill.

In Kincardine and Forfar, the forests of Alyth, Drymie, and Plater.

In Fife, the forests of Cardenie and Uweth.

In Stirling and Clackmannan, extenſive forests.

In Ayrſhire, the forest of Senecaſtre.

In the Lowlands the forests of Drumselch near Edinburgh; of Jedburgh, of Selkirk, of Cottonſhope, of Maldeſley, of Ettrick, of Peebles, of Dolar, of Traquhair, and of Melroſe. A

late as the 18th century. It is asked in "*the Surveiſors Dialogue*," (edition A. D. 1738, p. 187,) "To what place are you that are the tenants of this manner uſually called to do your ſervices, to muſter, and to ſhow your armour; and what beacons are you appointed to watch and ward at?"

* Some few additions have been made to this ſection ſince this paper was read at the Society, in conſequence of the information of which I have lately availed myſelf relative to the ancient forests of Caledonia, from a chapter in Mr Fraser Tytler's excellent *History of Scotland*. For ſome additions to my liſt of the earlieſt Scottiſh forest trees, I have to thank my friend Mr Neill.

forest, likewise, in David the First's reign, covered the district between the Leader and the Gala.

Such is the list of the ancient forests of Scotland, within the limits of which vitrified sites appear; and it is in vain to look for them where luxuriant woods have not subsisted. In the *Orkneyinga Saga*, for instance, we read of numerous beacon-signals having been lighted up in Orkney and Shetland; but as these islands from remote historic times have been destitute of forests, no fire has been raised of sufficient intensity to leave any marks of vitrification upon the mounds of stone on which the inflammable materials rested. * The same remark may apply to some of the Western Islands. Vitrification only appears where such mountain-fires have blazed as Olaus Magnus has described.

This very wooded state of Scotland, which was its peculiar character during the period when its shores were the most annoyed by the predatory excursions of the Northmen, will also, I trust, explain why vitrified sites should be so common in this country, to the exclusion of England, where, owing to the more early civilization of Saxon colonists, forest lands were beginning to be thinned, and where, from motives of necessity as well as convenience, the habit had commenced of using for beacon-fires vessels filled with pitch, instead of unwieldy piles of wood.

Keeping, then, this ancient wooded state of Scotland steadily in view, it is by no means illogical to extend rather than to limit the causes which would induce our ancestors in a country overspread with trees, where arable land was also much wanted, to allow the spoils of dense woods and thickets to be kindled upon every occasion of rejoicing, of religious sacrifice, or of alarm upon the approach of an invading enemy. All the ancient histories and traditions accordingly agree, that the most extravagant use was made of forest trees on every occa-

* This, my *late* remark, that there are no vitrified sites in Orkney, I have about a month ago corrected by the unexpected discovery of numerous vitrified mounds at Elsness in the island of Sanday, of which a notice, contained in a letter to Dr Brewster, is given in the succeeding article. This discovery is calculated to set at rest the most important questions relative to the origin of these vitrified remains.—S. II.

sion to which their burning could be applied. Until the thirteenth or fourteenth centuries, Scotland was in the exact state of Wales, which called for the particular law of Howel Dha. "There are," says this legislator, "three causes of progression for salutation; land brought into cultivation, festal games, and the burning of forests; and no person must obstruct such salutation."

In fact, the effects indicative of immense piles of blazing forest trees, the vitrifying action of which would be heightened by favouring currents of wind, as by a blast furnace of surpassing intensity, are most truly marvellous, oft-times appearing to vie with the result of volcanic incandescence. But the phenomenon will admit of explanation. The stones which are vitrified generally consist of such materials as granite, gneiss, mica slate, clay slate, or the older sandstones and porphyries; and these are rendered more or less fusible, in proportion to the felspar which they contain in their composition;—potash being an ingredient of felspar, occasionally to the amount of 14 per cent. The stony fragments which have been subjected to this vitrifying process often exhibit effects which are of particular value to the geologist, as they illustrate various circumstances connected with the agency of heat, in reference to rocks of much theoretical interest, which all ordinary experiments of the laboratory would fail in achieving. Thus, I might mention among the most interesting specimens which I collected from vitrified sites, those which show that heat has the power of causing strata of sandstone to assume the precise character of the more crystalline laminæ of gneiss, or of rendering gneiss itself prismatic.*

* The most beautiful and perfect specimens of prismatic gneiss I found some years ago among the vitrified ramparts of an islet (named the Burnt Island) in the Kyles of Bute. They were small, being about 4 inches in length, and about $\frac{1}{2}$ an inch in diameter, being formed at nearly right angles to the laminar planes of the rock.

Since reading this paper, I had the pleasure of observing, in the cabinet of M. Von Leonhard, the celebrated Professor of Mineralogy in the University of Heidelberg, several specimens from the vitrified sites of Scotland, which he had collected for the geological information that they conveyed to him of the effects of heat upon certain rocks. I also received a few weeks ago the Heidelberg new journal entitled the "*Jahrbuch für Mineralogie, Geognosie*," &c. conducted by Messrs Von Leonhard and Bronn,

The thirteenth or fourteenth centuries form the closing period to which we must limit the data of vitrified sites. The English, in their expedition against Scotland, endeavoured to clear the soil from its encumbering woods; and it is recorded that, in an expedition of the Duke of Lancaster, eighty thousand hatchets were heard resounding through the forests, which at the same moment were consumed by spreading fires. Lastly, as Mr Tytler has added, many districts were soon afterwards brought into cultivation, and converted into fields and meadowlands.

After the period of the destruction of Scottish forests, it would be futile to expect that any records would indicate the continuance of vitrifying causes. The hill which, as a signal of war, once proudly blazed with the lavish conflagration of stately trees, is now illumined with little more than a paltry tar-barrel! *Sic transit gloria mundi.*

Recapitulation.—But these investigations I shall now close by stating the conclusions to which I have thus far arrived. They are as follows:—

1st, That the notion that vitrification is the effect of volcanic agency, as well as the hypothesis which would consider vitrification as the result of a regular fabrication for the purpose of cementing stone walls, are conjectures equally chimerical.

2dly, That the theory of Lord Woodhouselee, that the conflagration of wooden ramparts by an assailing enemy might have produced the vitrification in question, is not established in any single instance; notwithstanding the probability that such a cause might have occasionally subsisted.

3dly, That the number of vitrified sites in Scotland is referable to the fuel extravagantly consumed during its ancient densely wooded state.

in the first number of which is an essay by the first-named gentleman upon the vitrified forts of Scotland. The scientific professor has reposed upon the theory of Williams, as revived by Dr Macculloch, which is, I believe, the only one known to continental geologists. There are several valuable observations of the writer on the effects of the artificial heat thus induced, when compared with the result of volcanic energy upon rocks of similar character. The resemblance, to which I have myself many years since adverted in various papers which I have read on vitrified forts, is often very striking.

4thly, That if we are entitled to suppose, that in a period of the history of Scotland when wood was most luxuriant, more than one ancient observance might have induced the vitrification in question, we are authorized in the expectation, that the character of the sites, in which vitrification occurs will be found as diversified as the multifarious national causes to which the effect may possibly be ascribed.

5thly, That some of the sites where vitrification is found were ancient places of rendezvous for tribes or clans upon any public occasion whatever of peace or warfare.

6thly, That many vitrified sites may from historical and internal evidence be shown to have resulted from beacon-fires formed by piles of wood, after the manner described by Olaus Magnus, Snorro, and other northern writers.

7thly, That other public occasions, festive or religious, might have given rise to the same effect of vitrification.

8thly, That it would be as easy to show, from numerous examples, that most of the oldest defences or Duns of Scotland exhibit no vitrification whatever, as that when vitrification does occur, it is not restricted to the precise limits of an area characterized by rude ramparts of stone.

9thly, That in some instances the vitrification of stony materials is of so small an extent, as to nearly elude the search after it; while in others it is of a continuity and intensity which almost surpasses credence.

10thly, That, as it cannot be proved that the vitrification in question is in every instance confined to fortified sites, *the term VITRIFIED FORT is too frequently the language of error.* And,

Lastly, That, as nothing can be more satisfactorily established, than that vitrification is an incidental, not a designed effect, the name of VITRIFIED FORT may with much advantage be exchanged for the more comprehensive and untheoretical one of VITRIFIED SITE.

ART. XVIII.—*Notice of the Discovery of very extensive Vitrified Remains at Elsness, in the Island of Sanday, Orkney.*

By S. HIBBERT, M. D., F. R. S. E., &c. In a Letter to Dr BRESWTER.

MY DEAR SIR,

To the communication which you have honoured me by inserting in your *Journal*, from the *Transactions of the Antiquarian Society of Scotland*, I have now to make an important addition.

In my observations on the theories which have been proposed to explain the vitrified forts of Scotland, you will find it stated, that “from the information of Snorro, we were entitled to expect that vitrification would be found on the mountain tops of Norwegian provinces; but that I was not aware that Scandinavian antiquaries had yet pointed out their existence.” I also remarked, in another article inserted in the *Archæologia Scotica*, (vol. iv. p. 184,) that the establishment of such a fact would throw no inconsiderable light upon the history of Scottish vitrification.

The solution of this question I have not yet obtained; but during the last month, I have arrived at the knowledge that vitrified sites exist in the ancient Norwegian colony of Orkney, which is quite as satisfactory.

During my visit to these islands, whence I am but just arrived, I must confess that I had but little expectation to find in it such a confirmed vitrified site as I have now to describe. Having many years since explored Shetland, the sister province of Orkney, and examined most of its ward or watch hills, without detecting any marks of vitrification upon them whatever; having also found in every topographical account of Orkney which I have consulted, a perfect silence regarding the existence of vitrified remains, I came to the conclusion (certainly a precipitate one,) that it was in vain to look for vitrified sites where luxuriant woods had not subsisted; that although we read in the *Orkneyinga Saga* of numerous beacon signals having been lighted up in Orkney and Shetland, yet that, as these islands, from remote historic times, had been destitute of

forests, no fire had been raised of sufficient intensity to leave any marks of vitrification whatever upon the mounds of stone on which the inflammable materials had rested.

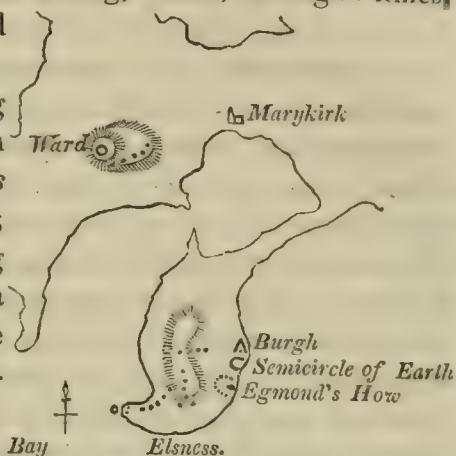
This conclusion I must now very materially qualify. It was in the museum of Mr Traill, of Woodwick, a scientific gentleman, who has formed a very interesting collection of the natural products of Orkney, as well as of its relics of antiquity, that I observed some very large specimens of vitrified stony matter, precisely like that which is obtained from vitrified forts, and with which you are familiar. These, Mr Traill informed me, had been sent to him by Mr Urquhart of Elsness, in Sanday, who had obtained them from the ness or promontory which imparts the name to his estate.

After these prefatory remarks, I shall proceed to describe the vitrified site of Elsness. You must, however, excuse me from doing more than giving you a very brief notice of it, as well as of the inferences, supported by historical documents, which are to be deduced from the discovery. These details, which would run to an unreasonable length, I have not yet had leisure to draw up in a connected manner. They will, in the course of the ensuing winter, as a continuation of my former memoirs, be laid before the Antiquarian Society of Scotland.

Elsness, lying to the south of the island of Sanday, is a promontory rather more than a mile long from north to south, and about half a mile broad. It was evidently the stronghold of a Scandinavian chief, one of the ancient sea-kings, being dignified by the presence upon it of the remains of a burgh, or circular fort, as well as of a large sepulchral tumulus, which bears the name of Egmond's How, and of a number of smaller cairns ranged near it in a semicircular form, which, perhaps, were likewise the ancient resting-places of the brave. Another contiguous site, which, by means of a low continuous mound of earth, is made to take the form of a large crescent, indicates by this particular structure the place of a *wcaponshaw*, or the site where a tribe was accustomed upon any hostile alarm to repair fully armed. Again, about three quarters of a mile to the north of Elsness, close to the ancient church named Mary Kirk, may

be traced the limits of an ancient ting, where, in Pagan times, the functions of the priest and the judge were combined.

But the most interesting remains of which Elsness can boast are the *beacon cairns* with which it is studded over;—many of these exhibiting unequivocal testimony of a vitrification quite as intense as is to be traced in any vitrified fort of Scotland.



These round cairns, of which I counted more than twenty, are from three to five yards in diameter, and elevated from two to three feet above the surface of the ground. The stone fragments, of which they are composed, which had evidently been collected from the beach, consist of what geologists would name an argillaceous schist; being, in this instance, an equivalent of the Mansfield slate. Their fusibility they have chiefly derived from the felspar, or rather the alkali, which they contain. The bituminous matter which may often be found to enter into their composition, and which, if constantly present, would materially add to their fusibility, is but an occasional occurrence.

Altogether, these mounds answer to the description given by Martin of the ancient beacons of the Isle of Harris, another early colony of the Norwegians: “There are,” says this writer, “several heaps of stones commonly called *Karnes* on the tops of hills and rising grounds on the coast, upon which the inhabitants used to burn heath as a signal of an approaching enemy.”

Upon the possibility, however, of mere heather to produce an effect, which I have been hitherto only disposed to attribute to the combustion of large piles of wood, I will not yet give an opinion. Wood is only found in Orkney in a fossil state, that is, buried in peat; and whether this substance was employed, or peat itself, which is by far the most abundant fuel in Orkney, or dried sea ware, or heather, or a mixture of two or more of these combustibles, I am not now prepared to dis-

cuss ; but shall merely remark, that the result produced upon the loose stones, which in the form of cairns supported the fuel, is most astonishing. In some instances, the vitrification has extended to the very bottom of a cairn, showing an almost entire compact mass. Nothing, in short, can display the effects exhibited more satisfactorily, than by contrasting them with the appearances induced on subjacent stones by the fires of the kelp-burners of Orkney ; where, if vitrification is at all produced, it is slight in the extreme, and rarely cements stones to an extent exceeding a few inches. This difference would indicate that a vitrification, in order to be considerable, must be a work of time, demanding that the same cairn for perhaps a century or more should be the unvaried site on which beacon fires were kindled.

The cairns of Elsness are not, however, all vitrified alike. On some of them I could not detect a single burnt stone, while in other instances a cairn would almost put on the appearance of one compact burnt mass. Too many of them also were concealed by a thick sward, so that their character for vitrification still remains indeterminate.

Such is the interesting vitrified site of Elsness, which I was as gratified in exploring, as upon finding that the rescue of its cairns from the unfortunate state of dilapidation, to which too many Orcadian antiquities have been subject, has been due to the very intelligent proprietor, John Traill Urquhart, Esq. of Elsness, who, desirous of ascertaining their origin, and fully aware of their importance to the archæologist, had given strict orders for their preservation. Previous to these injunctions, Mr Urquhart informed me, a number of these cairns, during the process of ploughing up an adjacent corn field, had been rooted up and levelled.

With these particulars of the actual appearances presented at Elsness, I shall for the present content myself ; and if any theoretical advantage is intended to be derived from them, it must be sought for amidst historical as well as local evidence ; but into this wide investigation I am precluded entering, no less on account of its length, than that it would involve details which are more calculated for the pages of Antiquarian Transactions than for your Journal. I shall therefore do no more

than glance at some of the conclusions to which I have arrived.

For three or four centuries, that is from the 10th to the 14th, the Scandinavian province of Orkney, always impatient of the control of the mother country, had no enemies to contend with so formidable as the kings of Norway, who frequently paid them hostile visits, to reduce them to submission. Against these incessant invasions the Orcadians were generally well prepared by keeping up a careful watch in their more northerly isles, which, upon the first approach of an enemy from the shores of Norway, should convey signals to a fleet anchored in a convenient port, and ready to put to sea, there to contend with its foes long before they could possibly land.

These simple historical circumstances are abundantly unfolded to us in the *Orkneyinga Saga*. Our inquiry, therefore, becomes comprised in the following questions; *First*, In what part of Orkney were its ancient galleys most commonly moored? And *secondly*, In what manner were timely signals conveyed to the fleet thus moored to arm and put to sea?

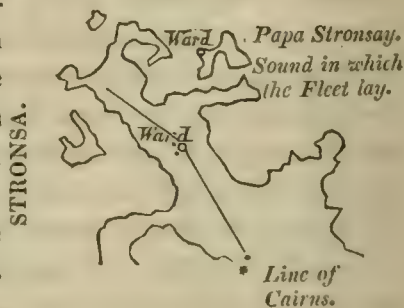
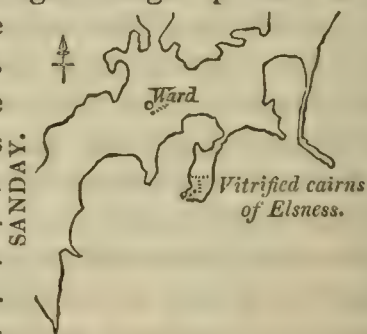
The first of these questions is soon resolved. It is evident, that, as hostile attacks were chiefly to be dreaded from the north, the most northerly harbour which could afford good shelter and depth of water for ships, provided also that it was situated on the east coast of Orkney, would be preferred; as these two circumstances of situation united, would be requisite for readily clearing out to oppose a hostile fleet, advancing in its proper course from Norway. Now, the most northerly island, lying also to the east of the Orkney group, is North Ronaldsay;—but here there is no harbour whatever. Nor is the island of Sanday, the next in succession, much more fortunate; its navigation being greatly obstructed by surrounding shoals of sand, whence the island has derived its name. In short, there is no port whatever which could have afforded any convenience to early war ships, required upon the approach of an invading fleet to instantly put to sea, more north than the sound of Papa Stronsa. This harbour, then, which lies due south of Elsness in Sanday, being divided from it by a channel a league and a-half across, must, from necessity, have been selected as the ancient Portsmouth of Orkney.

No other situation could have been so eligible for instant embarkation into the Northern Ocean ;—which superior advantage is even acknowledged at the present day, by its being the only harbour in the isles of Orkney which is deemed a convenient one for the prosecution of the North Sea Fishery of the Herring.

The site of the ancient Portsmouth of Orkney being thus established, the next object which I have is to shew through what medium telegraphic signals, which consisted of beacon fires, were conveyed to the fleet thus anchored in the sound of Papa Stronsa.

Shetland, which yielded a more willing obedience to Norway, was frequently in league with this power against Orkney, and as hostile fleets were often reinforced in the more loyal province, the intermediate island, named Fair Isle, of difficult access except to boats, was firmly retained by the Orcadians and converted into their most northerly signal station. From this site, an alarm fire, which would be first hailed in North Ronaldsay, would be answered by its inhabitants kindling a fresh flame in order that the intelligence might spread to Papa Westray and Westray on the west, and to Sanday on the south. Sanday would propagate the alarm to the fleet which was anchored in Papa Stronsa, with particulars of the number of hostile vessels approaching the Orcadian shores. These particulars, as we are assured by divers writers so late even as the time of Wallace, was usually signified by the number of fires which were lighted ; and hence the many vitrified cairns with which the signal station of Elsness in Sanday now appears studded.

In order also to complete the efficiency of this telegraphic system, every Scandinavian province



had its laws whereby watchmen were placed at the various wart hills of the country, as the Ward or Vord Hills of Orkney were named, who were required, under the severest penalties, to be constantly on the alert to transmit a signal of alarm to a fleet, or to the chain of beacons of which it might form a link. Accordingly, to the north of the small island of Papa Stronsa, a higher cairn than common, intended as a look-out place, appears, with the evident foundations of a building near it, which, I have no doubt, was the residence of the watchman whose office it was, upon the fires of Elsness being kindled, to instantly warn the fleet which was anchored in the contiguous sound.

The historical view which I have thus taken was familiarly, yet beautifully, illustrated when I visited Stronsa. On the opposite shore of Sanday several distinct kelp kilns were lighted up, which were well calculated to impress the imagination with a number of beacon fires, while the fishing-vessels then moored in the sound, like so many war ships, added a show of reality to the illusion. The number of the fires I could distinctly count. But the atmosphere was then clear. Under different circumstances, I can account for the disappointment occasionally expressed by the ancient Northmen, that the signals were not sufficiently explicit. In the year 1136, a messenger came to inform Earl Paul that war ships had been seen, but that it was uncertain whether the number was ten or twelve.

This is the general history of the vitrified cairns of Elsness. Yet I am still in doubt on one subordinate question, whether a telegraphic signal of the number of hostile vessels approaching, indicated by a corresponding number of cairns being lighted up, was communicated by this promontory from independent observations, or from signals transmitted from North Ronaldsay,—in which latter case we ought to expect as many vitrified cairns to be found here as at Elsness. I much regret, on account of the inconstancy of the weather, making a very hasty visit to this more northerly island. I was certainly shown by the well informed factor of this place, Mr Scarth, many cairns, the stones of which were discoloured by fire, yet there was none in which absolute vitrification was manifest. But I have requested the proprietor of the island, William Traill, Esq.

of Woodwick, from whom, on other occasions, I have received much valuable information of Orkney, to procure me more decisive satisfaction. In the meantime, I can only add, that it is highly probable that Elsness often derived its information of the exact number of approaching war ships from independent observation; as the high lands of Sanday afford nearly as good a look out station as North Ronaldsay.

After this particular examination, I visited several of the more common *wart* or *ward* hills of Orkney, but observed the beacon cairns upon them to show little more than discoloration from fire, with the exception of one ward hill only,—namely, that of Sanday, which is situated about two miles north of Elsness. Three of the cairns on this height were considerably vitrified.*

The circumstance of vitrification being chiefly observable on the beacon stations which connect themselves with the mooring of an armed fleet, while a similar manifestation in the ward hills of other islands of Orkney appears scarce, is of no little interest, and strengthens the conjecture, which is not altogether uncountenanced by history, that, upon the numberless occasions of invasion from Norway, it was much less frequently found necessary to alarm the country at large, than to confine the signal to the fleet which was in readiness; and that ward hills were only fired when an enemy was likely to make good its landing, or when it became necessary from any other circumstances to summon the whole of the islands to take up arms.

Nor is it of less moment to keep another incident in view:—that while Elsness from its peculiar locality was in every way adapted, by means of beacon fires, to communicate with the fleet of Papa Stronsa, the comparative lowness of the promontory prevented its signals from being observed by an expedition advancing from the north;—which fact, by the way, may explain many circumstances connected with the vitrified forts of Scotland.

Such is the general history of the vitrified cairns of Orkney, which may serve to set at rest, I trust for ever, two questions which have been agitated for more than half a century. The

* Since this communication went to press, I have received from Mr Traill of Woodwick, vitrified specimens from the island of Rousay, Orkney.—S. H.

first is,—To what uses or observances is the effect of vitrification attributable? While the second is, To what people is the effect attributable? In a tone of confidence, therefore, we are now entitled to reply,—That Vitrification was merely incidental to the fires which were kindled upon beacon stations; and that the people who in every country which they occupied or colonized, organized systems of beacon stations, were of Scandinavian origin.

You will also, my dear Sir, readily perceive, that the application of this conclusion to the theory of the vitrified forts of Scotland, (for I do not rank Orkney as a part of Scotland proper,) is a most important one. But I would not enter upon the disquisition here, which would be only to anticipate the memoir on the subject which I owe to the Antiquarian Society of Scotland, with whose encouragement my investigations on this subject have hitherto been uniformly honoured. If you will therefore accept my present crude impressions on this subject, I give them to you as follows:—

First, That the vitrified sites of Orkney not being characterized, as in Scotland, by the presence of stone ramparts, but simply by small cairns upon which the fuel for beacon fires had been placed, incontestibly shew, that a beacon station was not of necessity a place of strength or defence.

Secondly, That such of the ancient *Duns* or strengths of Scotland proper, in which vitrification is found to be an occasional occurrence, belong to the oldest fortified sites in the country, and are referable to some of its earliest inhabitants, probably to the Picts, who are supposed to be of German origin.

Thirdly, That these ancient *Duns*, not originally vitrified, indicate, by their construction and extent, that they were used by a people who had already passed from the hunting to the pastoral state; as they evidently comprehend in their design the protection of cattle, with that of human defence.

Fourthly, That from the tenth to the fourteenth century, a considerable part of Scotland was overrun by the Scandinavians under the various names of Northmen and Danes, who reciprocally became themselves liable to invasion from other piratical tribes of the same northern origin as themselves, and were therefore induced to institute systems of beacon fires, in imitation of those with which they had been familiar in Norway.

Fifthly, That as in most instances the ancient fortresses or Duns of the oldest historical period of Scotland, were continued to be used as the gathering-places of clans or tribes, the same were most conveniently selected as the sites of beacon fires; the ramparts of loose stones, which characterize such fortified sites, serving the additional purpose of cairns on which the fuel was placed.

And, *sixthly*, That the intensity of fusion exhibited on these vitrified sites is no less referable to the forest trees which on such occurrences extravagantly blazed, than to the incessant hostile invasions which caused beacon fires to be lighted.

But while I state these my *present* conclusions, (for it is possible I may yet make some little modification in them,) I would not renounce the idea, that other public occasions, as, for instance, the annual lighting up of the fire of the Belting, might have assisted, though in a subordinate degree, towards producing the vitrified effects, which continue to be the astonishment of all who are conversant with their extent.

But I must now conclude, as my letter has already exceeded the limits which I had at first assigned to it. The interest which continental geologists have begun to take in the varied effects of ignition which these vitrified sites display, has naturally made them curious regarding their mysterious history; which circumstance is the only apology I can offer for making your Journal on this occasion a medium of pure antiquarian inquiry. In some future Number, I hope to, render you acquainted with the mineralogical observations of M. Von Leonhard on the specimens examined by him from the vitrified sites of Scotland.—Believe me, &c. &c.

EDINBURGH,

S. HIBBERT.

August 30, 1831.

ART. XIX.—*Some Notices regarding Vanadium.* By JAMES F. W. JOHNSTON, A. M., &c. Communicated by the Author.

I. *Process for obtaining it from the Vanadiate of Lead.*

The mineral which I have called Vanadiate of lead is a compound of arsenic, phosphoric, muriatic and vanadic acids, with oxide of lead, and a small admixture of earthy bases,—the se-

paration of the phosphoric and vanadic acids *quantitatively* from each other is attended with difficulties which I have not yet been able wholly to overcome. The metal, however, may be extracted by the following simple process: The mineral is dissolved in nitric acid, with which it forms a yellow solution. From this acid solution the lead and arsenic are thrown down by a current of sulphuretted hydrogen. The solution is now of a beautiful blue, and contains the vanadium in the state of oxide. It is filtered and digested for some time in a gentle heat to separate the sulphuretted hydrogen, and a trace of sulphuret of arsenic remaining in solution. Evaporated to dryness by a gentle heat, it changes from blue to green, and leaves a beautiful deep red mass, which is chiefly vanadic acid. This is boiled in successive portions of a perfectly saturated solution of carbonate of ammonia. The colourless liquor is filtered while hot, and deposits on cooling a white crystalline powder in minute prisms, which is a vanadate of ammonia. This salt may be further purified by again dissolving in distilled water and crystallizing. By exposure to the air it becomes after some days slightly yellowish, probably from the loss of a portion of ammonia. Heated in the open air, this salt parts with its water and ammonia, and leaves oxide of vanadium, which, by a continuance of the heat, combines with the oxygen of the atmosphere, and forms vanadic acid. The yellow powder thus obtained fuses by an increase of temperature into a deep red liquid, and crystallizes on cooling into beautiful prismatic radiating needles, sometimes of a deep red, but generally of a steel gray colour, and high degree of lustre. Fused in considerable quantity in a covered crucible and cooled gradually, it gives beautiful large purplish transparent crystals transmitting a brownish light. These crystals are pure vanadic acid, mixed when blue or grey with more or less oxide of vanadium. By heating the salt in close vessels or in an atmosphere of carbonic acid, an indigo blue, or, when rubbed in a mortar, a dark olive brown powder is obtained, which is the oxide of vanadium.

II. Metallic Vanadium and its properties.

I have not been able to effect the reduction of the oxide by
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means of hydrogen gas. Berzelius, at a white heat, obtained a slightly cohering mass, which he considered to be but imperfectly reduced. Both the oxide and the acid are reduced easily when heated with potassium or sodium, with the evolution of much heat and flame, but without detonation. But the metal is easiest obtained by mixing the oxide, or the chloride prepared by evaporating the muriatic solution to dryness, with oil, and forming it into a ball. This is placed in a small charcoal crucible—surrounded with powdered charcoal—placed within one or two other crucibles, and heated to full whiteness for two or three hours. By this process it is reduced, but not fused, being in the state of a grayish, slightly cohering, powder. To procure it in a *massive* state, the oxide must be previously fused in a platinum spoon, the crystalline mass surrounded with charcoal and oil, and heated for several hours. The mass thus obtained has a metallic lustre, and possesses the following properties:—

1. It is of a reddish-white colour, approaching to that of bismuth, so hard as to be with difficulty affected by the file; and brittle, giving, when rubbed down, a grayish powder.

2. It does not affect the magnetic needle, but is an excellent conductor of electricity.

3. Heated in the flame of a lamp before the blowpipe in a pair of forceps, or on charcoal, or to a full red heat in a platinum spoon, it burns with a visible incandescence, and becomes indigo-blue or steel-gray, exhibiting on the surface traces of partial fusion. It undergoes no further change unless the heat be long continued, when it fuses into a red liquid, which on cooling below redness contracts in volume, and shoots at once into beautiful radiating star-like groups of acicular crystals, occasionally of a reddish, but generally of a steel-gray colour, and high degree of lustre.

4. Heated to incipient redness in an atmosphere of oxygen gas, it burns, emitting a brilliant reddish light, and is converted into an indigo blue oxide of a crystalline structure, which fuses with difficulty in a platinum spoon over a spirit lamp.

5. In chlorine gas it burns at a heat below redness, forming a greenish yellow vapour, which condenses into a deep brown-

ish red volatile liquid. As it escapes into the air the vapour is decomposed, forming white fumes of muriatic acid and vanadic acid (?), which deposits itself in the form of a red crust round the mouth of the tube by which the vapour escapes. Confined in a moist receiver, it is changed into a muriate, and a beautiful blue liquid is formed, which is a solution of the oxide in muriatic acid.

6. Heated in atmospheres of sulphur and iodine, it undergoes no change.

7. In nitric acid it dissolves rapidly, with effervescence and evolution of red fumes, forming a deep indigo-blue solution, which, by spontaneous evaporation, gives an amorphous partly steel-grey and partly brownish crust, which is a mixture of the vanadic acid with the oxide of vanadium. Evaporated to dryness by a gentle heat continued till fumes of nitric acid cease to be given off, it leaves a beautiful vermilion red vanadic acid, which is slightly soluble in water and forms a yellow solution. Heated further, in the open air, this red substance fuses into the red liquid already mentioned.

8. Neither the sulphuric nor muriatic acids act upon it sensibly in the cold, whether dilute or concentrated, and very slightly even when aided by heat. The oxide is dissolved by both acids, giving beautiful blue solutions resembling those of the salts of copper.

III. *Distinctive Characters of Vanadium.*

This metal is most nearly allied to chromium, and it cannot easily be confounded with any other. It differs from chromium as follows.

1. The protoxide of chromium is green and insoluble in water, that of vanadium is dark brown and soluble. The deutoxide of chromium, or the compound of oxide and acid is brown; vanadium appears to give a similar compound, which is of a beautiful indigo-blue. *

2. Newly precipitated oxide of chromium dissolves very

* Sefström speaks of a green oxide, (*An. de Chim.* Jan. 1831.) This I have not obtained, unless, indeed, we suppose it present in the green solution occasionally obtained in acid, and which I suppose to be mixture of the blue solution of the oxide and the yellow solution of the acid.

sparingly in solutions of caustic and carbonate of ammonia, with a rose red* colour; the oxide of vanadium dissolves easily in ammonia, giving a translucent dark brown or olive solution, from which it cannot be precipitated by alcalies, as oxide of chromium can.

3. Both acids are red and fuse into a red liquid; but while the chromic is changed at a higher temperature into the infusible oxide, the vanadic, according to Sefström, parts with a small portion of its oxygen, and crystallizes on cooling, as already stated.

4. The chromic acid never loses its colour, neither by heating nor by combinations with the bases. A solution of the vanadic occasionally loses its colour by heating; and forms, with excess of base, colourless salts similar to the vanadate of ammonia, mentioned above. The presence of vanadic acid may be at once detected in any of these colourless salts or solutions by a drop of nitric acid, which reddens the salt and makes the solution yellow. The crystallized acid or *bi*-vanadiates are not to be distinguished in colour from the *bi*-chromates.

5. Both metals give volatile chlorides, but that of vanadium, as already stated by Berzelius, (*An. de Chim.* xlv. p. 332,) is not always coloured. If a mixture of a *chromate* and a chloride be heated with sulphuric acid in a tube sealed at the one end, a beautiful red gas is given off, which condenses if the finger be kept on the mouth of the tube into a beautiful red liquid. A similar mixture of a *vanadate* with a chloride heated in the same way gives a colourless gas.

6. Solutions of the vanadiates give, with the metallic salts *in general*, precipitates resembling those given by the chromates. The salts of lead and mercury form the most striking exceptions. Newly precipitated chromate of lead is of a bright yellow, which it retains when dry, forming the beautiful and well known pigment. The vanadate of lead, at first bright yellow, is bleached by the action of light, and becomes occasionally pure white. Were it plentiful, therefore, it could never take the place of the chromate in the arts.

* Gmelin's *Handbuch*, i. p. 845.

With proto-nitrate of mercury, chromates give a *deep red*; vanadiates a *bright yellow* precipitate. Both give with nitrate of silver a ruby or vermilion red, differing only in shade.

In these differences in the properties of these two substances, having towards each other so many and striking affinities,—we have ample means both of distinguishing and of accurately separating them from each other, should they ever be found in combination: and though, as the number of simple bodies with which we are acquainted becomes greater, the properties of the whole group will more and more shade into each other, yet the number of points in which these two metals disagree, shows us there is yet ample room for an addition to their number without rendering the difficulties which oppose the analytical chemist absolutely insurmountable.

I have not proposed to myself the difficult task of collecting materials for an entire history of vanadium and its compounds, knowing it to be already in far abler hands. Berzelius has been occupying himself with it during the past winter, and he has in all probability already finished his researches; but so little has yet appeared in this country regarding the new metal, and it may be yet so long before the results of Berzelius find their way to our journals, that I have thought the above sketch of its properties might in the meantime not be uninteresting to British chemists.

An "Old Correspondent," in the August number of the *Philosophical Magazine and Annals*, p. 157, mentions my having found vanadium in an ore of lead from Alston Moor, and suggests that it was more probably from the neighbourhood of Keswick.—On this I have only to remark, that I have never found it, and never said it was to be found in any ores of lead, except the two varieties described in the former number of this *Journal*, page 166, as formerly occurring in a now unwrought mine at Wanlockhead.

PORTOBELLO, 8th September 1831.

ART. XX.—*On the Nature of the Light in the Two Rays produced by the Double Refraction of Quartz.* By G. B. AIRY, M. A. M. G. S. Late Fellow of Trinity College; Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge; and Fellow of the Cambridge Philosophical Society. (Abridged from the *Cambridge Transactions*. Read February 21, 1831.)

I PROPOSE in this paper to offer some conjectures as to the nature of the light forming the two rays produced by the double refraction of quartz; to describe the experiments on which they are founded; and to explain the calculations by which the theory and the experiments are compared. The subject is one to which (I believe) no attention has been paid, except by one distinguished foreigner; the mode of calculation is original to me, and is, to the best of my knowledge, new.

It is well known that the rays produced by the double refraction of calc spar, (calcareous spar, Iceland spar, or rhombohedral carbonate of lime) and most other doubly refracting crystals, are entirely polarized: one in the principal plane passing through the ray (or, if a biaxial crystal, in the plane equally inclined to the planes passing through the ray and the two axes) and the other in a plane perpendicular to the former. From the exact agreement of the phenomena of depolarization with the calculations made on this hypothesis, we are justified in supposing that the law holds true when the rays are so little separated that it is difficult to observe them in the common mode of inspection. Now it has generally been supposed that the two rays of quartz are polarized in the same way: differing from those of calc spar only in the magnitude and direction of their separation. It was known, however, almost as soon as Arago and Biot commenced their observations, that there is some anomaly in the rays passing in the direction of the axis of quartz; and the latter of these observers established the difference of right-handed and left-handed quartz. Fresnel by a simple experiment, (see Note A.) (which I have repeated) showed that the light in the direction of the axis of

quartz is not one ray, but two rays moving in the same direction, and with different velocities. He showed, moreover, that a new kind of light may be produced by causing polarized light to undergo two internal reflexions in a glass rhomb with certain angles, the plane of polarization being inclined to the plane of incidence at an angle of 45° ; and that this light is exactly similar to one or other of the two rays above-mentioned, according as the plane of polarization is on one or the other side of the plane of incidence. And by a mathematical investigation, of which I am unable to supply the deficient steps, he showed that the effect of the internal reflexions is to retard by one quarter of an undulation the undulations perpendicular to the plane of incidence, so that in the light thus modified the particles of ether which were originally in a straight line will at any time be found in the form of a circular helix, and each will revolve uniformly in a circle. (See Note B.) And from the nature of the original experiment it appeared that in right-handed quartz it is necessary to suppose the right-circular polarization transmitted with the greater velocity; in left-handed quartz the contrary. I have repeated and varied most of Fresnel's experiments relating to this subject, and am perfectly convinced of the correctness of his views.

Now if, in the experiment with the glass rhomb, the planes of polarization and incidence be inclined at any other angle than 45° , the magnitudes of the undulations parallel and perpendicular to the plane of incidence will no longer be equal: but the alteration of their periods will be the same as before. The displacement of the particles of ether will still be represented by a helix, but instead of being traced round a circular cylinder, it must be supposed traced round an elliptic cylinder. This modification may properly be called (as Fresnel has called it) elliptical polarization. (See Note C.) This term has since been used by Dr Brewster to express the nature of the light (probably indetical with this, or nearly so) reflected from metallic surfaces. Should any difference be found, I have no hesitation in fixing on the modification above described as that to which in ought in propriety to be attached.

I am now able to explain my conjectures on the nature of the light in the two rays of quartz.

1. I suppose the ordinary ray to consist of light elliptically polarized, the greater axis of the ellipse being perpendicular to the principal plane; and the extraordinary ray to consist of light elliptically polarized, the greater axis of the ellipse being in the principal plane.

2. I suppose that when the ordinary ray is right-elliptically-polarized, the extraordinary ray is left-elliptically-polarized; and *vice versa*.

3. I suppose that the proportions of the axis of the two ellipses are the same: each proportion being one of equality when the direction of the ray coincides with the axis, and becoming more unequal, according to some unknown law, as the direction is more inclined to the axis: the minor axes of the ellipses having sensible magnitudes when the rays are inclined 10° to the axis.

4. I suppose that the course of the rays after refraction can be determined by the construction given by Huyghens for calc spar, with this difference only, that the prolate spheroid for determining the course of the extraordinary ray must not be supposed to touch the sphere for determining the course of the ordinary ray, but must be entirely contained within it.

These conjectures were originally suggested by the desire of finding some connecting link between the peculiar double refraction in the axis discovered by Fresnel, (see Note D,) and the double refraction commonly recognized. All the phenomena of colours which I have observed agree perfectly with the results of my hypotheses.

I may mention that I have found observations corresponding to many detached parts of the phenomena which I have viewed assembled, in the early memoirs of Arago and Biot. But the method which these philosophers used, (particularly the latter,) of examining a small part only at a time, does not appear to be well adapted to the discovery of the laws of light. In the experiments which I am about to describe, every thing depends on the form of the coloured curves; and to attempt to discover this from observation of detached parts would be perfectly hopeless. These coincidences I have recognized only since I made my own observations.

It must be observed that all the phenomena mentioned below

are described as they appear when examined with an analyzing plate of unsilvered glass. If a plate of tourmaline be used, the right and left parts of the image will have the same relative position, but the upper and lower will be interchanged: the observer's eye being supposed to turn in such a manner that the axis of the tourmaline appears *up* and *down*.

Phenomena.—I. If a plate of calc spar cut perpendicular to the axis be examined with the polarizing and analyzing plates crossed, the uniaxal system of rings is produced. If the analyzing plate be turned less than 90° either way round the incident ray, the complementary system is produced. The order of colours does not sensibly differ from Newton's scale, beginning with black. These are common and well known phenomena.

II. If Fresnel's rhomb of glass be placed to receive the polarized light, so that the plane of reflexion pass through the divisions 45° and 225° , the calc spar will present another appearance. The rings are abruptly and absolutely dislocated: those in the upper right hand quadrant and the quadrant opposite to it are pushed from the center by one-fourth of an interval, and those in the other quadrants are drawn nearer to the center by the same quantity. The line separating the quadrants is nowhere black: the intensity of its light is uniform and almost equal to the mean intensity. If the plane of incidence pass through 135° and 315° , the phenomena of adjacent quadrants are exactly interchanged. No alteration is made by turning the analyzing plate round the incident ray: the lines dividing the quadrants are always parallel and perpendicular to the plane of reflexion at the analyzing plate. (See Note E.)

III. If the plane of reflexion in the rhomb pass through 0° and 180° , or through 90° and 360° , the phenomena are precisely the same, and undergo the same changes as those in Phenomenon I. If, while the plates are crossed, the rhomb be turned gradually from the position 0° towards 45° , the rings are gradually changed, at first becoming (as far as the eye can judge) elliptical, and then assuming the form represented in Fig 8.

IV. If a plate of quartz, whether right or left-handed, be interposed between the crossed plates, a set of rings is seen. As far as the eye can judge, the rings are exactly circular, but there

is no black cross, and the central tint is not black, but removed from it by a number of tints in Newton's scale proportional to the thickness of the quartz. Thus, with a thickness 0,48 inch, the central tint is pale pink : with a thickness 0,38 inch, the central tint is bright yellowish green : with thickness 0,26 inch, it is a rich red plum-colour : with thickness 0,17 inch : it is a rich yellow.

The colours then appear to be nearly the same, beginning from the center, as in Newton's scale, beginning with the tint representing this central tint. At a considerable distance from the center four dark brushes begin to be visible, in the same directions as the arms of the black cross in calc spar.

V. Now (supposing the crystal right-handed), if the plate of quartz be thin, and the analyzing plate be turned, the upper part towards the observer's left hand, a bluish short-armed cross appears in the centre, (see Note F,) which on turning further becomes yellow : and the rings are enlarged. On turning still further, the cross breaks into four dots. The rings are no longer circular, but of a form intermediate between a circle and a square, their diagonals (as well as the cross) being inclined to the left of the parallel and perpendicular to the plane of reflexion. See Fig. 9. If the analyzing plate be turned the other way, there is no cross : the form of the rings is changed from circular nearly as in the former case.

VI. If the plate of quartz be thick, the dilatation of the rings and the change of form are all the perceptible phenomena. And on turning the analyzing plate continually to the left, the rings continually dilate, and new spots start up continually in the centre, and become rings. If the crystal be left-handed, the remarks on this and the last article apply equally well, supposing the analyzing plate turned in the opposite direction.

VII. If Fresnel's rhomb be placed in the position 45° , and the light thus circularly polarized pass through the quartz ; on applying the analyzing plate, instead of rings there are seen two spirals mutually inwrapping each other as in Fig. 10. If the rhomb be placed in position 135° , the figure is turned through a quadrant. If the quartz be left-handed, the spirals are turned in the opposite direction. The central tint appears to be white. With the rhomb which I have commonly used (which is of

plate glass, but with the angles given by Fresnel for crown glass) there is at the centre an extremely dilute tint of pink: I think it likely that this arises from the error in the angles, as the intensity of the colour bears no proportion to that in other parts of the spirals. The figure was drawn from the appearances given by a plate of quartz 0,36 inch thick.

VIII. If two plates of quartz of equal thickness, but cut one from a right-handed and the other from a left-handed crystal, be attached together, and put between the polarizing and analyzing plates, the left-handed slice nearest to the polarizing plate, the appearance presented is that of Fig. 11. Four spirals (proceeding from a black cross in the centre, which is inclined to the plane of reflexion) cut a series of circles at every quadrant. The points of intersection are in the plane of reflexion, and perpendicular to it. This is the simplest way of describing the form: but if we followed the colours which graduate most gently, we should say that the form of each is alternately a spiral and circular arc, quadrant after quadrant. At a distance from the centre the black brushes are seen. If the combination be turned so that the right-handed slice is nearest to the polarizing plate, the spirals are turned in the opposite direction. This is one of the most beautiful phenomena of optics. The slices from whose appearance the figure was drawn are each 0,16 inch thick.

[Mr Airy next proceeds to explain the mode of calculating these phenomena on assumed laws of the nature of light in the two rays of crystals; but as this investigation occupies thirty-five pages, intelligible only to the mathematical reader, we are obliged to refer such readers to the original Memoir.]

From the agreement between the observed and the calculated appearances, I think there is little doubt that the nature of the light in the two rays of quartz is such as I have described. I do not mean to exclude the possibility of supposing that the form of neither wave (in the construction for determining the course of the rays) is exactly spherical or exactly spheroidal; provided the difference of the forms be nearly the same as that of a sphere and a spheroid. Nor do I mean to assert that each elliptically-polarized ray consists exactly of two plane polarized rays following each other at the interval of one-fourth of an un-

dulation: or that the ratio of the two axes in the two rays is exactly the same. But I conceive it to be perfectly certain that the general character of the light is such as is stated in my hypotheses.

I have not made any calculations upon other suppositions, but I can hardly imagine that any other would represent the phenomena to such extreme accuracy. I am not so much struck with the accounting for the continued dilatation of circles, and the general representation of the form of spirals, as with the explanation of the minute deviations from symmetry, as white circles become almost square, and crosses are inclined to the plane of polarization. And I believe that any one who shall follow my investigations and imitate my experiments, will be surprised at their perfect agreement.

There is one relation between the construction for determining the course of the rays, and the nature of the rays, which deserves (I think) particular attention. It is that (comparing the rays of quartz with those of any other crystal) a change in the nature of the ray is accompanied with an interruption of continuity. The *nappes* of the wave surfaces are absolutely separated. This is not the case in the common construction for uniaxal crystals, nor in Fresnel's construction for biaxal crystals. There may possibly be a connection of the same kind as that between the change from partial reflexion to total reflexion within glass, and the accompanying change from plane polarized light to elliptically-polarized light. The cases are at least thus far analogous, that the change in the light and the interruption of continuity go together. But we are so much in the dark respecting the physical constitution of quartz, that we cannot at present go farther.

It might have been desirable to verify my suppositions by more direct experiments on the separate rays of quartz. I can only plead that the duties of my office have not allowed me the necessary time. They would (under all circumstances) have been much more troublesome than those which I have the honour of laying before the Society: and I do not think that they would have been more satisfactory. The appearances presented by depolarization are admirably adapted to the discovery of the most delicate differences in the nature and course

of rays. The same want of time I hope will be allowed as an excuse for the want of accurate measures: without which, no theory, however satisfactory in general explanations, can be considered as firmly established.

Note A.—It is not easy to make this experiment in a satisfactory manner. If the axes of the crystals are not precisely adjusted, several images will be seen. I have not succeeded in obtaining two only, though I have made the others much more faint than the two principal.

Note B.—As I cannot appreciate the mathematical evidence for the nature of circular polarization, I shall mention the experimental evidence on which I receive it. *1st*, The light when received on an analyzing plate or tourmaline presents the same appearance in whatever direction the analyzing plate is turned round the incident ray. *2d*, The phenomena of depolarization are the same in whatever direction the analyzing plate is turned. *3d*, If the polarized light passes through two such rhombs placed in similar positions, the plane of polarization is shifted 90° . *4th*, If they are placed in crossed portions the plane of polarization is unaltered. *5th*, The phenomena of depolarization agree with the calculations founded on this supposition: in uniaxal crystals, where the plane of polarization of one ray is changed 360° in going round the axis, the alternate quadrants are pushed in and thrust out one quarter of a tint: and in biaxal crystals, where the plane of polarization is changed only 180° in going round the axis, the alternate semicircles are altered in the same manner.

Note C.—If I might venture to fix on the discovery of Fresnel, which among all his wonderful additions to optical science appears likely to possess the greatest practical value, I should select his invention of the mode of producing circularly-polarized or elliptically polarized light by internal reflexion of plane-polarized light in glass or water. He has given us the power of producing light whose laws are as well known as those of plane-polarized light, and which is more manageable, inasmuch as it admits of degrees in its ellipticity. The beautiful geometry of Malus is forgotten when we think of the discovery of polarization: the far more valuable theoretical discoveries of Fres-

nel will lose their pre-eminence when put in competition with an invention which enables others to make discoveries.

Note D.—It does not appear, I think, that Fresnel had made any distinct supposition as to whether the two rays in the axis should be considered as the ordinary and extraordinary ray in their ultimate state, or not. From all that I could extract from his Memoirs, I was always in doubt whether both the ordinary and the extraordinary ray in the neighbourhood of the axis ought not to be considered as divided each into two circularly polarized rays.

Note E.—It is proper to mention that I had exhibited this phenomenon to the Cambridge Philosophical Society in the spring of last year, long before the publication of Dr Brewster's valuable Memoir in the *Phil. Trans.* for 1830, and (I believe, but I do not recollect the date,) before its communication to the Royal Society.

Note F.—This may be considered as the definition of *right-handedness* of the crystal: and this observation gives the readiest means, with a thin plate, of determining whether it is right-handed or left-handed. If the plate be thick, the easiest method is to observe in which direction the analyzing plate must be turned to make the rings dilate.

OBSERVATORY, CAMBRIDGE,
Dec. 30, 1830.

ART. XXI.—*Description of a new Species of Lamprotornis.*

By JOHN BLACKWALL, F. L. S., &c. Communicated by the Author.

THERE have been recently deposited in the Museum of the Society for the promotion of Natural History established in Manchester, two specimens of a bird belonging to the genus *Lamprotornis*, which, notwithstanding it is one of the most superb species of its tribe hitherto discovered, appears to have escaped the notice of ornithologists. The following description will serve to convey some idea, though it must be admitted a very inadequate one, of this highly interesting and beautiful bird:

Order,	<i>Insesores,</i>	Vigors,
Tribe,	<i>Conirostres,</i>	Cuvier,
Family,	<i>Sturnidæ,</i>	Vigors,
Sub-family,	—————?	
Genus,	<i>Lamprotornis,</i>	Temminck.

L. Vigorsii. Bill and legs black ; plumage soft, silky, and glossy ; head, sides, and back of the neck, upper part of the back and lesser wing-coverts bright green, tinged with gold ; scapulars and lower part of the back of a rich bronze-yellow ; greater wing-coverts, feathers of the spurious wings, tertials, and the tips and upper part of the outer web of the primary quills deep purple, relieved with violet, blue, and gold ; lower part of the exterior web of the primaries dull green ; inner webs of the primaries and secondaries black, faintly clouded with purple, those of the four longest quill feathers in each wing being abruptly emarginated near their termination, a prominent point, formed by the projection of the more elongated fibres of the web, rendering the sudden transition in its breadth remarkably conspicuous ; outer webs of the secondary quills black, with a bright tint of bronze-yellow ; tail rounded at its extremity, black, with a slight mixture of bronze-yellow above, particularly on the middle feathers ; tip, and outer edges of the lateral feathers purple, glossed with violet, blue, and green ; upper and under tail-coverts purple, varied with violet, steel-blue, and green ; cheeks purple, tinged with green ; throat and anterior part of the neck and breast similar in colour to the scapulars, but less brilliant ; abdomen bronze-green, tinged with yellow ; thighs and flanks purple, clouded with violet and steel blue ; under side of the wings and tail black. Colour of the eyes not known.

Total length, $11\frac{1}{10}$ inches ; bill, from the point to the forehead, $1\frac{9}{10}$; from the point to the gape, $1\frac{1}{10}$; wings, from the carpus to the tip of the fourth quill feather, $6\frac{1}{10}$; tail, $4\frac{7}{10}$; tarsi, $1\frac{7}{10}$; middle toe, including the claw, $1\frac{2}{10}$.

As the two individuals of the above species in the Manchester Museum were imported to this country from Brazil, along with a considerable collection of skins of Brazilian birds, it is possible that this elegant creature may be indigenous in South America. I have no positive information, however, that such is the fact ; and M. Temminck, in treating upon the family to

which it belongs, (see his *Manuel d'Ornithologie*, vol. i. p. lv. note (3),) remarks, that “ toutes les espèces sont de l'ancien continent, le plus grand nombre d'Afrique.”

I have named this splendid bird in compliment to that distinguished and disinterested naturalist, N. A. Vigors, Esq. who politely directed my attention to it as a species new to ornithologists.

In detailing those peculiarities of structure which characterize the genus *Lamprotornis*, M. Temminck says of the toes, “ l'interne soudé à sa base, l'externe divisé,” (*Manuel d'Ornithologie*, vol i. p. lvi.) the very reverse of what is actually the case. This error, which probably originated in inadvertency, has been recently repeated by a celebrated French zoologist; it becomes the more desirable, therefore, that it should be corrected.

ART. XXII.—*Observations on a Pamphlet, entitled, “ On the Alleged Decline of Science in England. By a FOREIGNER. London, 1831, Pp. 33.”* Accompanied by a Preface by M. FARADAY, Esq. F. R. S., &c.

“ It would be unwise in a foreigner to give an opinion on matters which he cannot be expected to understand.”—*Pamphlet*, p. 16.

THE present dispute respecting *the Decline of Science in England*, presents to us the rare and almost ridiculous feature, never before exhibited in literary controversy, of *all* the combatants on one side coming openly forward with their names, and pledging their public characters for the truth of their facts and statements; while *all* those on the other side are anonymous assailants, fighting behind black crape, or discharging their missiles from the masked batteries of magazines and reviews. *Sir H. Davy, Mr Herschel, Mr Babbage, Sir James South, and Professor Leslie*, have all lamented, in powerful and eloquent terms, the decline, and ineffective state of science in England;—and though their allegations have been deemed by ignorant and interested individuals, injurious to the honour of the country, yet not one man bearing the name either of a philosopher, or even of the lowest grade of authorship, has ventured to place his name and personal character in opposition

to the able and honest and patriotic expostulations of these great men. Having expended their munitions of war in special pleadings, in the grossest perversion of well known facts, the heroes of the mask have at last been driven to the expedient of subsidizing a scientific foreigner. We regret to find that so excellent and eminent an individual as Mr Faraday has been the Chargé d'Affaires who has negotiated this loan of exotic talent; but though it is gratifying to find that he abjures the idea of having by this act "expressed an opinion on the subject either one way or the other;" yet it will be seen from an expression in his preface, which we subjoin, that he has more than identified himself with the author on the general question, however anxious he may have been to renounce the heavy responsibility of having given an editorial sanction to those severe animadversions on the Institutions of France, and on the personal characters of her most distinguished philosophers, which constitute so remarkable and so unpleasant a feature in the present pamphlet.

"The MSS," says Mr Faraday, "in English, of the following series of arguments was sent me by the writer, an eminent scientific foreigner, in consequence of a few remarks in our mutual correspondence upon some recent publications in England. Without being considered as expressing an opinion on the subject either one way or the other, I am still desirous of placing my friend's reasons before the public; not merely because no one can judge correctly who has heard but one side of a question, but also as a great literary curiosity; for all must allow that it is an extraordinary circumstance for English character to be attacked by natives and defended by foreigners.

"I have scrupulously refrained from altering the language; preferring rather that a few quaint expressions should remain than that so distinctive a characteristic should be lost.

"For the clearer comprehension of certain passages, I ought to say that it has been in my possession at least four months.

August 1831.

M. FARADAY."

When Mr Faraday remarks that "it is an extraordinary circumstance for *English character to be attacked by natives*

and defended by foreigners," he declares in his own name that Sir H. Davy, Mr Babbage, Mr Herschel, Sir James South, Professor Leslie, &c. &c. who maintain the decline of science, have *attacked English character*, and he has thus unwittingly let out the whole secret of the bitterness which a certain class of scientific men have evinced in the present controversy. The *English character* here referred to, must mean the character of those English philosophers who may be regarded as the representatives of English science, and who may be supposed entitled to feel that its decline has been owing to their indolence or incapacity: But unfortunately for Mr Faraday's allegation, the principal representatives of English science are the very persons who are here supposed to attack English character, and therefore Mr Faraday must mean that Sir H. Davy, Mr Babbage, &c. have attacked his scientific character, and that of the other individuals, who like him maintain that science has not declined in England. The question is therefore no longer one of fact but one of feeling; and we are presented with the singular circumstance of the principal philosophers of England maintaining and lamenting the decline of their favourite sciences, while, generally speaking, some of the philosophers of the lower class are asserting that their characters are attacked by the allegation, and that science was never in a more prosperous and thriving condition. If this question were to be submitted to the scrutiny of an English jury, we have no doubt that the testimony of Mr Babbage and his supporters would be received as that of men who sacrifice their feelings of personal and national vanity in the great cause of truth, while the evidence of their opponents would be rejected as that of partisans, disqualified from judging of a subject in which they had acknowledged their own personal characters to be involved.

From Mr Faraday's preface we now pass to the pamphlet itself, the first pages of which are occupied in expressing the surprise of the author that, while it has been customary for rival nations to assert their respective pre-eminence in knowledge, "a disposition entirely different is observable among some of the *most scientific men* of England. They appear bent upon undervaluing their own country, and seem to take a *secret* (not very secret surely,) and certainly a strange

“*delight* in extolling to the skies the scientific excellence of “foreigners of every description.”

Overlooking the gross perversion of truth involved in the expression that Mr Babbage and his friends have *taken delight* in extolling foreigners to the skies, we shall have no difficulty in explaining to an English reader, how men passionately attached to their country and to its scientific honour, may yet point out the defects of its institutions, and give a salutary warning of the decline of its literature and science. When a man embraces every opportunity of abusing the institutions of his country, and evinces a pleasure in extolling the superiority of foreigners, while he expresses no wish to reform these institutions, and to raise his countrymen in that scale in which he considers them as holding a degraded place, it is not difficult to determine the character, and appreciate the motives of such a calumniator. The sharp-sighted public sooner or later detect the fraud, and the intellectual felon, the robber of his country's name, is pronounced guilty of acting under irritable feelings, disappointed hopes, or personal animosity.

Mr Faraday and his anonymous friend will, we are sure, save us the trouble of proving that Sir H. Davy, Mr Babbage, and Mr Herschel, &c. do not belong to this class of persons. They have, on the contrary, shown themselves to be men jealous of the honour of English science, and we could prove from their writings, and those of their supporters, that they have nobly vindicated the claims of English pre-eminence whenever they were assailed by the unjust pretensions of foreign authors. Sir H. Davy was upon this point extremely sensitive, and Mr Faraday ought to have been aware from his personal knowledge of that bright ornament of England, that a philosopher may admit the decline of science in his own country, while he at the same time exercises a morbid jealousy of the pre-eminence of rival nations. It was, indeed, this true love of English science, this anxious desire to maintain its pre-eminence, that led Sir H. Davy to deplore its decay, and lament the indifference which the aristocracy and the government of England have exhibited in promoting our intellectual glory. Mr Babbage, and Mr Herschel, and Sir J. South, have been actuated by the same noble and patriotic motive. They struggled in vain to remove the defects in our scientific institutions,

by their personal and individual exertions ; and appealed only to public opinion when other means had proved unavailing. The evil management of the Royal Society, and its baneful effects upon science, were exposed, not to lacerate private feelings or promote private ends, but to advance the character of an institution whose history was interwoven with that of the brightest period of British science. The decay of mathematical and physical knowledge was pressed upon public notice, not to depreciate the character of English philosophers, but to raise them to new exertions to revive and extend the one, and to raise and ennoble the other. The splendour of foreign talent was brought forward, not to derogate from the lustre of British genius, but to exhibit the happy influence of Royal favour and of national institutions ; and the liberality of foreign governments was justly placed in painful contrast with the shallow and ignoble policy of our own, with its habitual neglect of intellectual worth, with its exclusion of genius from all the offices and honours of the state, and with its more than barbarous indifference to the true glory of a civilized empire.

Such were the undoubted motives of the reformers of science, and we have reason to know that many of its youthful cultivators have not only justly appreciated them, but have considered the declining state of English science as a ground for more strenuous exertions to maintain the honour of the nation. Many of our readers will probably bear testimony in their own hearts to the existence of such a feeling, and we trust that there is a numerous class of scientific men who will cherish the noble sentiments so well expressed by Mr Brayley in the following commentary upon Mr Babbage's views, which seems to have been composed for the very purposes of our argument. It occurs in a well written work, "*On the Utility of the Knowledge of Nature, considered in reference to the General Education of Youth.*"—London, 1831.

"Strong representations," says he, "have of late been made, and among other competent advocates, by the consummate mathematician who now occupies the chair in the University of Cambridge, once filled by Newton, of the paucity, in this country, of high mathematical knowledge, as well as that of interest in the more elevated pursuits of those sciences which are immediately connected with it, such as astronomy, and

“ the more recondite departments of material physics. Our
 “ continental neighbours, as well as the philosophers of Ger-
 “ many and the north of Europe, have been held up as ex-
 “ amples for our emulation, in this respect, which, it has been
 “ stated, we must for the present be content to follow at an
 “ humble distance. *That these representations are found-*
 “ *ed in truth with respect to the particular branches of know-*
 “ *ledge in question, cannot be doubted ; and that they should*
 “ *be true, every lover of science in Britain will regret, while*
 “ *he will feel himself called upon to use his most strenuous en-*
 “ *deavours to remove this blemish from our national honour.*”

These are the sentiments of a generous and unprejudiced mind, which regards the sacred interests of truth as paramount to all private feelings ; and we must request Mr Faraday, after reading the above passage, to decide upon the respective merits of those who repel all friendly admonitions and wholesome truths as insults to their character, and of those who receive them in simplicity of heart, and apply them to the great purposes of personal and national improvement.

Having thus vindicated Mr Babbage and his friends from the unwarrantable imputation of having “ *attacked English character,*” we shall proceed to examine the *defence of English science* by a *Foreigner*. This amiable individual has performed with admirable tenderness, the united offices of apothecary and sick-nurse to scientific England. When none of her own faculty durst visit her in her decline, save the few empirics who have administered to her in the dark, our kind devotee, under a mask, is conducted by Mr Faraday to soothe and to rouse the deserted patient. Elixirs, moral and physical, are plyed with exquisite address,—morphium and henbane combine their narcotic virtues, and new emollients and stimulants are extracted from every corner of the materia medica. It is alleged, says he, from the very chair of Newton in the University of Cambridge, that mathematics, once the staple commodity of England, has greatly declined, and Mr Herschel has dared to say, that in this science, “ *England has long given over a hopeless race.*” How can this be, when Mr Herschel and Mr Babbage are both fitted to run, and when La Place has praised Mr Ivory, and M. Arago praised Dr Young ? “ *There is not the slightest*
 “ *reason for giving over the race as hopeless, whilst such men*

“ as Mr Ivory, Mr Herschel, and Mr Babbage, are in the full
 “ force of their talent. In France, M. Legendre is very old,—
 “ the venerable Lacroix has done more than any other living
 “ man for the diffusion of analytical science ; *we see no one at*
 “ *present in France but M. Poisson* who could enter the lists of
 “ the race. I do not perceive, I repeat it, the least necessity
 “ for giving it over.”

Here we have our author fairly drawn from his generalities into a distinct proposition incapable of being misinterpreted. He starts English mathematicians, of whom he considers only THREE as *fit to run*, viz. Mr Ivory, Mr Herschel, and Mr Babbage, against French mathematicians, of whom he considers only ONE as *fit to run*, viz. M. Poisson. In order to make up this strange equation, he eliminates M. Legendre because he is *old*, and M. Lacroix because he has done more than any other living man, and he leaves M. Poisson to strive single-handed with the *three* English mathematicians. We now beg the reader's attention to the ignorance and unfairness of this contrast, which is as reprehensible as a piece of reasoning, as it is unjust to the high merits of the French geometers.

Mr Ivory, now approaching the end of a brilliant career, is just as much disqualified for pursuing analytical research as Legendre or Lacroix ; and both Mr Herschel and Mr Babbage have long ago abandoned their mathematics, the one for astronomy, optics and chemistry, and the other for mechanics, so that the English list of our author is already reduced to *zero*, and his argument falls, self-destroyed, to the ground.

But what shall we say of our author's knowledge, when he assumes M. Poisson as the only French mathematician. Has he not heard of *Cauchy*, one of the brightest ornaments of France, and one who is likely to revive the age of Laplace and Lagrange. Does he exclude Biot, Poinsot, Ampère, Puissant, Prony, Navier, and Damoiseau, from the right of competing with the mathematicians of England ? He will doubtless reply that many of these philosophers have chosen other fields for the principal display of their talents ; but he knows well that the same was the case with Herschel and Babbage, and he ought, therefore, to have omitted their names on the opposite side.

So far for our author's flattery of English mathematics.

We think we may leave this point to Mr Faraday's own decision ; and though we do not entirely agree with the low opinion of our mathematical character expressed by Mr Rennie, Professor of Natural History in King's College, London, in his Introduction to Montague's *Ornithological Dictionary*, yet we know, that such opinions are very generally maintained by unprejudiced, though possibly not very well informed, individuals. " The consequence of this leading-string system," says Prof. Rennie, " is, that it has nearly extinguished the mathematical reputation of Britain, formerly so high ; it being as impossible to make a Newton by parroting the *Principia*, as to make a Milton by committing to memory his *Paradise Lost*."

Another singular argument of our anonymous opponent deserves the special notice of the logician, and all dealers in syllogism.

" The names," says he, " of Sir Humphry Davy and of Mr Herschel are, of course, of the highest authority ; but it would appear rather strange, that any one should attempt to couple those names with a complaint of a decay of science. This sounds pretty much as if, when speaking of Wellington and Nelson, one would argue on the inferiority of the British navy and army. The mere mentioning of the names of Davy and Herschel, could furnish a proof that science was flourishing in that country which gave birth to these highly gifted individuals."

When this syllogism is arranged, *quoad artem*, it thus stands. Sir H. Davy and Mr Herschel declare, that science is declining in England ; and they are persons of the highest authority ; but they were both born in England, therefore science must be flourishing in place of declining !! Now for the illustration of the syllogism. The British army and navy never can be inferior to those of other nations while we can speak of Wellington and Nelson ; therefore science never can decline in England while we can speak of Davy and Herschel. Upon these principles, we are led to the cheering conclusion, far more delightful than that of the author, *that science and the army and navy never can decline in England*, because Davy and Herschel were born within its limits, and because we can always speak of Wellington and Nelson. This

perversion of reasoning will appear still more prominently when we consider that science never can decline except in a country where it has flourished, and which has given birth to great men ; so that the very circumstance which must necessarily have preceded the decline of science, namely, the birth of two great men such as Davy and Herschel, is held by our author as an irrefragable proof that science has not declined.

In order to follow the other arguments of our author, we must explain the circumstances under which science may be said to decline in any country.

1. Science must decline by the death, especially the premature death, of one or more of its most eminent and active cultivators.

2. Science must decline, when the principal scientific institutions of a country cease to be presided over, and managed by distinguished men, and are conducted in a slovenly routine, and in a way in no respect calculated to encourage genius or promote the advancement of science.

3. Science must decline when the sovereign and his ministers take no interest in scientific pursuits,—when, in place of establishing new scientific institutions, they destroy old ones,—when the honours and offices of the state are conferred only on military and naval merit,—and when those duties which require to be discharged by men of scientific knowledge, are entrusted to persons who have no other claim but those of family connections and political subserviency.

4. Science must be considered as declining in any country, even *when none of the preceding causes exist*, provided it is making less progress than formerly in that country, either in the number or nature of its discoveries, or provided its progress does not keep pace with that of rival nations.

Admitting the most favourable views of the talents of British philosophers, and the importance of British discoveries, there is no upright and well informed man in the community, who will not acknowledge with bitterness and sorrow that *from all the preceding causes, science has declined, and is declining, in England.* The truth of this sad conclusion, which has been fully established in Mr Babbage's work, and in the *Quarterly Review* for October 1830, will appear from a brief view of the subject.

1. The death of Sir H. Davy, Dr Wollaston, Dr Young, Mr Watt, Dr Marcet, Mr Gregor, Dr John Murray, Mr Chenevix, and Mr Smithson Tennant, within a short time of each other, was almost a death-blow to English science, and particularly to the science of chemistry; and it behoves those who allege that science, and especially chemical science, has not on this account declined, to favour the public with the list of those who fill the shoes of those great men, or of the young aspirants after chemical fame, who hold out the promise of placing themselves in the wide breach so unexpectedly effected.

In order to answer this argument, our learned foreigner resorts to the following singular artifice, in which he unintentionally admits its full force.

“In chemistry,” continues Mr Herschel, “the case is not much better. But to us foreigners it appears hardly just to complain of the state of chemistry, when the losses of Davy and Wollaston are still so recent. With the exception of Berzelius, whose mind, however, is of a quite different turn, we will ask Mr Herschel what continental chemist can be compared to the two eminent men whom England has to lament?”

Now this is a truly admirable counterpart to our author's former reasoning. When *he* wished to estimate the condition of French mathematics, he cuts off Legendre as old and infirm, and therefore forming no fraction even of French talent; but when *we* wish to estimate the condition of English chemistry, he will not allow us to cut off Sir Humphry Davy and Dr Wollaston after they have been years in their tomb! Nay, he insists that no foreign chemist but Berzelius can be compared with them, and yet he will not permit us to bewail their loss as an immediate and irreparable injury to science.

When we maintain that science has declined in consequence of the loss of such great men, we surely do not mean to undervalue those whom they have left behind. We, on the contrary, value them the more, and we implore, with double earnestness, their eminent successors, Mr Dalton, Dr Henry, Dr Thomson, Mr Phillips, Dr Prout, Dr Turner, Mr Johnston, and others, to come forward with new zeal, and call forth new powers to restore chemistry to its wonted glory. As Mr Faraday considers his character attacked by the allegation of

the decline of chemical science, it is in vain to appeal to his individual patriotism.

2. The influence of institutions, whether universities or societies, in promoting or retarding the progress of science, is universally admitted. The author of the present pamphlet, to use a proverbial expression, admits this "with a vengeance," when he declares that the establishment of the *Ecole Normale* in Paris, "*though of short duration, was perhaps of more utility towards the extension of mathematical knowledge* THAN "ALL THE UNIVERSITIES OF EUROPE TOGETHER." All that Mr Herschel and Mr Babbage have written on the low condition of English mathematics, and all that we and others have said of the inefficiency of our universities, sink into insignificance compared with this heart-stroke against the honour and the usefulness of these institutions. The author of this sentiment, who certainly cannot be a professor, must, if he quits his mask, keep at a respectful distance from Cambridge, for if the corporation spirit of this university, when subjected to the gentlest admonitions of its true friends, has already bled where it was not wounded, and winced where it was not galled, what powers of wrath will it not deploy against the daring author of this alarming truth, or the flagitious fabricator of this monstrous calumny? That Cambridge, backed by all the universities of Britain, and by all the universities of Europe, has done less for mathematical science than the short-lived Normal School of Revolutionary France, is indeed an assertion calculated to excite the patriotism of the most unpatriotic, and to awaken the indignation of the most phlegmatic Englishman.

In thus stumbling among our venerable institutions in pursuit of the Lucasian professor, our foreign instructor blunders out a series of the bitterest though well meant remarks. "Most of the English institutions," says he, "*seem of so particular a nature, and so blended with other subjects totally different from what exists in other countries, that it would be unwise in a foreigner to give an opinion on matters which he cannot be expected to understand.* * * * * But this certainly *is a principle firmly entertained on the continent, that the diffusion of knowledge is far better promoted by having the sciences taught in the universities by first rate*

“professors than by any universitarian regulations.” But notwithstanding these hard hits our author is such a lover of things as they are, that he tells us “that in this as in many other things *the best plan* seems to be to let things take their natural level and not interfere in every particular with legislative provisions.” He very soon, however, repents of this toleration of abuses, for in a few lines, when he gets hold of an abuse that he really understands, he seems resolute enough to have it rectified.

“It is generally understood,” says he, “that there are mathematical and astronomical professors both at Oxford and Cambridge who *seldom or never* lecture, and who often do not reside at all at the university. I am possibly blinded by continental prejudice, but to me this appears a glaring abuse, which elsewhere would call for a speedy remedy.”

Having his eyes thus opened by one intelligible abuse, our author begins to see too sharply. He attacks the Duke of Wellington in the most tender point, and levels a general blow at the institutions of our country, which, if it is true, or even approximates to truth, presages the speedy downfall of mighty England. If there is one feature in our institutions more noble than another, or more flattering to our national vanity, it is the accessibility of high office to character and talent. It has ever been our boast, and we trust it long will, that the humblest individual may, by the force of education and character, rise to the highest offices that a subject can fill—to the Archbishop of Canterbury—the Woolsack, and the Horse Guards. Our author, however, believes and asserts the contrary.

“Mr Babbage,” says our author, “illustrates his argument by asking what would have been the military renown of England, if, with an equally improvident waste of mental power, its institutions had forced the Duke of Wellington to employ his life in drilling recruits, instead of planning campaigns?” To this we might answer, *omnis comparatio claudicat*. But admitting the simile, it would have been a great waste of mental power certainly, if the whole life of the noble duke had been employed ‘in drilling recruits; but it must be remembered that the greatest captain of the last century, Frederick the Great, amidst all his various occupations, literary and political, always employed part of

“ his time in that exercise which Mr Babbage deems so far
 “ below the genius of a Wellington. Perhaps if the illus-
 “ trious British commander had contented himself during the
 “ last years with superintending the discipline and drilling of
 “ the Grenadier Guards, instead of figuring at the head of
 “ government, he would have rendered an essential service to
 “ his country, not attended with a loss of that popularity
 “ which was once so deservedly bestowed upon the conqueror
 “ of Waterloo. But we cannot help remarking, that the case
 “ which Mr Babbage very justly reprobates actually exists in
 “ England. If Lord Wellington had not been what he is,
 “ the son of the Earl of Mornington, but the son of a farmer,
 “ a common soldier, like the present king of Sweden, Ney,
 “ Lefebure, Soult, Murat, Lannes, and similar men, he might
 “ have been, according to the institutions of England, doomed
 “ for life to the drilling of recruits, but he never could have
 “ been an officer.”

On first catching the substance of this paragraph, it is difficult to resist the impression that the author really united the incongruous characters of a scientific and a radical reformer ; and, but for the appendage of the responsible name of Mr Faraday, we should certainly have regarded it as a hoax upon the credulity of Englishmen. The attack upon the Duke of Wellington is most unmerited, and we think we speak the sentiments of all parties when we say, that but for one imprudent demonstration against a great popular measure, his merits as a statesman will bear a comparison even with his military greatness. But how shall we reply to the astounding charge, that the institutions of England are such that none but the sons of Earls can become even officers ! Silence is the best refutation of a charge so palpably untrue, and we shall only ask our author how, under such a system, scientific men could either expect or obtain rewards.

From the state of our Universities, of which we have allowed our author alone to speak, we pass to the state of our Royal Society ; and we shall consider whether or not it has suffered such a change as to lead to the inference that science may on that account have declined.

Within a short period previous to the death of Sir H. Davy, this distinguished individual filled successively the offices of

Secretary and President to the Royal Society. Dr Wollaston in like manner filled the offices of Secretary, President, and Vice-President; Dr Young was its foreign Secretary, and Mr Herschel was its Secretary.

It was scarcely to be wondered at that the Royal Society was alleged to have declined, when death or resignation had deprived the institution of these its brightest ornaments. In filling up these sad blanks, despair rather than sagacity seems to have presided at the election. It might have been expected that the salaries of three of these appointments should have been given to some young and active philosophers whose income, limited either by circumstances or by a too generous devotion to science, required such an addition. But with a fatality which marks a falling institution, the three secretaryships were conferred upon men who, though in every way estimable and respectable, had never been particularly active in original research; and all of whom enjoyed good professional incomes. One of these secretaries, indeed, was so loaded with pluralities that it required *seven* lines of Mr Babbage's smallest type to record their names; and another was actually *an officer of artillery on leave of absence from his regiment*, and who also held the paid situation of scientific adviser to the admiralty. Of such a state of things it was the natural consequence that the rewards of the society were adjudged upon new and erroneous principles, that its *Transactions* no longer shone with original discovery, that its funds were devoted to promote no scientific object, and that the managers of the institution, deserted by some of its brightest ornaments, were nearly incapable of forming an opinion of some of the papers submitted to their review. Of subsequent events we forbear to speak; they have a tongue of their own which will soon bring them into public notice.

If any person who peruses this statement shall refuse to admit that the Royal Society has declined, and science along with it; we would invite him, forgetting for a moment space and time, to sit down, as we have done, at the same table with Cavendish, Sir W. Herschel, James Watt, Maskelyne, Playfair, Hutton, Davy, Wollaston, Young, and Chenevix, these mighty dead, and then to name the party of the mighty living to which he will invite us in return.

3. The supineness of the sovereigns and cabinets of England, and the indifference of the aristocracy to the great objects of science, have been so fully treated in Mr Babbage's book and in the article on the decline of science in the *Quarterly Review*, that we shall confine ourselves to a reply to the observations on these subjects made in the pamphlet under consideration.

Our author admits "that Sir H. Davy's remark, "that the "aristocracy may be searched in vain for philosophers," may "be very just; and it is indeed to be lamented." He adds, "that noblemen, as the Earl of Macclesfield and the late Duke "of Marlborough, do not find many imitators amongst the "English nobility." Desirous, however, of finding some exception to Sir H. Davy's observation, he afterwards remarks, "that it must not be forgotten that the first poet of the age " (Lord Byron) rose to such literary eminence from a seat in "the British House of Lords;" as if this had anything whatever to do with the subject under discussion. "Perhaps the "cause of this neglect," he observes, "may be traced to the "circumstances that individuals moving in those higher spheres "of society *may obtain those distinctions*, (the honours of the "peerage,) *which are objects of honest ambition, at a more "easy rate than by labouring in the field of science.*" If this is not a well merited sneer at our mode of conferring honours, it indicates great simplicity and ignorance on the part of an author. He surely does not believe that any labours in the field of science, however brilliant in themselves and useful to the nation, could by any possibility obtain such honours; and he cannot but know that *the easy rate* to which he refers is nothing more than the possession of great wealth, the exhibition of military or naval prowess; the dominion over rotten boroughs, and the maximum of political subserviency.

But, notwithstanding all these admissions, the author will not allow "that science is therefore declining in England, "unless it could be shown that the same reproach attaches to "other classes of society." He is of opinion that the English people are great patrons of science, "and that science has "a better chance in England than elsewhere of securing an "honest independence to its possessor. Dr Wollaston," he continues, "by his scientific exertions, procured himself that "pecuniary supply without which the greatest genius and

“ the ignorant are alike unable to support themselves. Now
“ I will tell Mr Babbage, that in no country Dr Wollaston,
“ unassisted by kingly favour, could have been able to earn by
“ his scientific discoveries, that independence which gave him
“ the necessary leisure to apply all his mental force to the
“ pursuits of science.”

Our author here commits a mistake which we have found a very common one, and which, therefore, requires to be corrected. England is a wealthy, commercial, and manufacturing country, and any person, whether a native or a foreigner, who brings into her markets an article of trade which administers either to our wants or our luxuries, is sure of disposing of it to the best advantage. Dr Wollaston discovered the art of rendering platina malleable, and long enjoyed the monopoly of supplying this valuable metal. Hence he made a fortune by it, in the same manner as Dr Solomon did by his Balm of Gilead. Mr Watt made a fortune by the steam engine, but many other persons have done the same who made no great improvements upon it. In all these cases the fortune was made by *trade*, not by science, for if Dr Wollaston had found his art in an old book on alchemy, or had purchased it from a foreign chemist, he would have made the same fortune which he did. Sir H. Davy's far greater discovery of the metals of the earths never obtained for him a single farthing, because it had no commercial value.

But while the wealth of England will be exchanged for an article of trade, whether it is the product of science or of quackery, the laws and customs of England are arrayed against scientific exertions. Had Dr Wollaston taken a patent for his method of purifying platina, he would probably never have made a farthing, and might have been reduced to poverty by so rash an act. Had he generously presented his method to the British public, and thus thrown a new trade into the hands of the community, the government would never have awarded to him either thanks, or honours, or remuneration. He was therefore compelled, to the great injury of the country, to keep his method a profound secret till he lay upon his death-bed, and but for accidental circumstances, it would have perished with him. Dr Wollaston, therefore, made his money in spite of his country, and solely because he could

bring his invention into the market without disclosing the secret of it.

There are few inventions, however, of such a nature that they can be sold without disclosing the principle of their construction. Of these Dr Wollaston's *Periscopic Spectacles* was one. He was in this case compelled to secure it by patent. The patent was invaded by pirates, and he lost the just reward of his labours. Now, in every other country in Europe, this patent would have been protected, and though these countries cannot boast of such extensive bazaars as London, and could not have supplied so many purchasers for new spectacles, yet it is easy to decide which of them would have been the greatest patron of the inventor. As another proof that science is well rewarded in England, our author mentions the case of Sir W. Herschel, who, he says, "left a handsome, "and what elsewhere would be considered a large fortune." Does the author know that Sir W. Herschel's fortune was that of the lady whom he married? If he does, why does he pervert truth, and if he does not, why does he write on a subject which he does not understand? In continuance of our last quotation our author goes on to say,

"I know such countries where the high-minded Wollaston
"might have been obliged to fawn and bow in the anteroom
"of some lawyer in office, where his discoveries would have been
"submitted to the examination and criticism of some official
"underling; of having his writings and experiments appre-
"ciated by those who neither can conduct an experiment nor
"weigh the force of an argument; and after submitting to all
"these indignities—after having felt the full weight of the in-
"solence of office, he might have had the humiliation to see
"preferred to his just claims, the unceasing importunity, and
"the shameless effrontery of the impudent quack, and of the
"subservient sycophant. This, I will tell Mr Babbage, is the
"real state of scientific men in those countries, whose manner of
"managing scientific concerns he affects to rate so much above
"that of his own country, and *nisi exempla essent odiosa*, we
"might bring convincing proofs of the truth of our assertion,
"from the scientific history of almost every country in Europe."

True as we doubt not this picture has sometimes been on the Continent, yet we will tell our author that it is, and has been

pre-eminently true of England, and that, if he wishes to see his own description justified or even caricatured, he will succeed only by submitting some important invention to the British government. As an anonymous writer, he need not have scrupled to give us a few of the *exempla odiosa* to which he alludes; but, however, numerous they might have been, we should have neutralized them by counter statements of the most striking kind. When Dr Anderson, disgusted at home, carried his flying artillery to the French government—when Fresnel successfully submitted his improvements in light-houses to the French board,—when Aldini, introduced his incombustible dresses—when Humboldt persuaded the Russian government to erect physical observatories throughout the Empire,—when Hansteen received from the Storting of Norway a large sum for examining the magnetic curves in Siberia,—when the Emperor of Austria, the King of Russia, and the King of Saxony invited to the honours of their capitals, the scientific associations of Germany,—was there in these acts any appearance of official insolence to genius and talents? We call upon our author to vindicate his allegation by facts, and we doubt not it will be found that the cases referred to, were those of troublesome speculators who abound in all countries.

But not only are foreign sovereigns the liberal friends and patrons of indigenous science, their liberality, especially that of the Emperor of Russia, nobly extends itself to other countries; and while we are writing these lines, we have learned the gratifying fact, that his Imperial Majesty, has presented to George Harvey, Esq. F. R. S. of Plymouth, through his Excellency Prince Lieven, a magnificent diamond ring, as a mark of approbation of Mr Harvey's investigations on ship-building, as published in the article Ship-building in the *Edinburgh Encyclopædia*.

We come now to a painful part of our duty; to notice the calumnies against French philosophers and French science, which the author has brought forward to neutralize the high admiration which Mr Babbage and the author of the article in the Quarterly Review have expressed of the genius and institutions of the French people. He represents the "wealth and dignities" which Napoleon heaped upon Laplace as "the price of shame and degradation;" and he enumerates *Lacépède*, *Monge*,

Berthollet, and Cuvier, among the philosophers, "all of whom," he says, "bartered their conscience for wealth and influence." He afterwards adds, with additional bitterness, "Such men as Laplace, Lagrange, Lacepede, Cuvier, and Fontanes, enjoyed certainly a high income; the former were senators, whose salary was 36,000 francs, or L. 1500; but it is a glaring error to call this salary an *emolument of science*. It was the price of the shame and degradation incurred by these men for giving their silent vote and sanction to any of Napoleon's decrees, however oppressive; it was the price for which the silence and connivance was bought of these mock commissions for the *liberty of the subject* and the *liberty of the press*, which subsisted as sinecures during Napoleon's reign. Supposing the English ministry thought it their interest to bring scientific members into Parliament, would Mr Babbage consent to sell his vote and his conscience for a pension of L. 1200 or 1500? Still this was what La Place, La Grange, Lacepede, Monge, Berthollet, &c. constantly did, and we must not expect to buy at a cheaper rate the honour and reputation of men of such distinction."

Human nature shudders at such imputations upon men whose names and characters have become the property of mankind; and while we express our firm belief that they are false, we must add also our regret that they should have received currency in a pamphlet published in England, and edited by an English chemist, who, honoured with a seat in the Institute of France, owed to that distinguished body at least the homage of his silence. This is doubtless the time when every patriot would wish to draw closer the ties which are beginning to unite two mighty nations, and we earnestly hope that every other feeling will be extinguished but those which spring from an honourable rivalry in promoting the best interests of our species.

Before quitting this branch of the subject, we must notice two extraordinary mistakes of our author. He supposes that the contribution of L. 2740 annually, by 685 members of the Royal Society, is a *subscription for the encouragement and benefit of science*, and that it is a greater honour to be a member of the Royal Society than a member of the Institute of France.

"A very material difference," says he, "between the Royal Society and the Institute, consists in this, that the fellows of

“ the former pay for their admission, whilst the members of the
 “ latter enjoy a pension. Now, there are at present 685 fellows
 “ of the Royal Society, who used to pay about L. 4 a-year for
 “ the honour of their fellowship, making in all an aggregate
 “ sum of L. 2740, or 68,500 francs. The members of the
 “ French Institute receive, or received formerly, each 1500
 “ francs, making 10,250 francs of the public money. Now, we
 “ will ask Mr Babbage in which country the honour of belong-
 “ ing to the first scientific society is held in greater estimation,
 “ in that in which the fellows pay the honour with 68,000
 “ francs, or those where they receive 10,250 francs, and the
 “ honour as an addition besides?

“ Mr Babbage favours us with a table of the costs of fellow-
 “ ship of different learned societies; it would have been very
 “ curious if he had stated the annual sum thus contributed both
 “ by the English and French nations towards the encourage-
 “ ment and benefit of science; and from the result, a fair esti-
 “ mate might be drawn in which of the two countries science is
 “ held in the greatest honour.”

He who subscribes L. 4 a-year to the Royal Society, receives in return the *Philosophical Transactions* gratis, the pleasure of attending the meetings of the Society, the soirées of the president, the chance of belonging to the Royal Society club, and all the advantages of a reading room and library, which is open almost every day of the year from 11 A. M. till 4 P. M., advantages really worth more than the subscription. Nay, there have been cases where booksellers have exerted themselves, whether successfully or not we do not say, to obtain the title of F. R. S. for their hack writers, and have considered the subscription as balanced by the commercial advantages which it secured. The man must, therefore, be insane, who imagines for a moment that such subscriptions are contributions to science. Nay, the subscriptions are altogether expended in publishing the *Transactions*, and in paying taxes to the government and other contingencies, and are actually returned in another form to the pockets of the members. The medals, &c. of the Society are all paid out of legacies left for the purpose, and the Society has never assisted a single scientific man in carrying on experiments, and has never appropriated its own subscriptions for any scientific object. The same remarks are applicable to the Royal Society

of Edinburgh,—with this difference, that it has on several occasions, though not on so many as it ought, devoted its funds to promote important scientific objects. We can assure our anonymous flatterer, that there is not one of the 685 members whom he mentions who really conceives that he is an encourager or benefactor of science.

With respect to the comparative honour of being a Fellow of the Royal Society, and a Member of the Institute, we can scarcely believe that the author is serious. Every person must feel that it is an advantage to be a member of the Royal Society, but none that it is an honour. The indiscriminate admission of every applicant of respectable character who enjoys title, possesses wealth, or fills office, and the strange but undeniable fact that no persons but men of science find it difficult to be admitted, are circumstances which exclude all ideas of honour as connected with the transaction.

In the case of France, the honour of being an Academician is so palpable, that it is universally admitted, and that honour is certainly increased by its being accompanied with a salary. In London the members purchase their place as a matter of barter; in Paris the academician is purchased by the government as a commodity valuable to the state. If our readers are not satisfied with this view of the subject, we must bring the argument more home to their apprehension. Mr Herschel and Mr Babbage consider the honour of being connected with the Royal Society as so evanescent, that they have omitted the “Ten pound a-piece letters” from their names. We may add our testimony, that we feel no honour but much advantage from the connection; and we request Mr Faraday sincerely to acquaint his friend whether he feels himself most honoured by being a Fellow of the Royal Society or a corresponding Member of the Institute.

4. The fourth proof of the decline of science, namely, that which is drawn from the actual state of science in this country, opens up a field too extensive to be discussed at present. It has been partly treated under other heads, and we shall only appeal to our readers whether Mr Herschel, and Mr Babbage, and Sir James South,—or Mr Faraday and his correspondent, are best fitted to judge of the state of the physical sciences in England, and which of the two parties are actuated by the purest and most

patriotic motives in the opinions which they have maintained respecting the decline of science. The motives of men are generally not cognizable by human judgment ; but circumstances often present themselves, in which motives are as palpable as the actions to which they lead. When Mr Babbage, Mr Herschel, and Sir James South sounded the alarm of declining science, did they sit down with folded arms to witness its decay. On the contrary, they were, and still are, the most ardent and active philosophers in England ; and, what is more, they are all men perfectly independent in fortune, seeking for no official emoluments, but generously devoting all their time, and much of their fortune, to sustain and revive those very sciences of which they have lamented the decline. The occupations and the motives of these three great men are peculiarly under public observation ; and though it is perhaps unfair to separate one from the rest, we cannot but express our admiration of the noble and unrivalled sacrifices which have been made by Sir James South to promote the progress of astronomical science.

But who are the patriotic defenders of English science ; the panders to our national vanity, the calumniators of genius, who represent our first philosophers as *Firebrands*, and ascribe to disappointment, and irritability and faction, the warnings and expostulations of the best friends of England ? Let them stand forth ! let them unmask their persons ! and let *him* especially present himself to be scanned by the eye of truth, who dared to say to the philosophers and inventors of England, what the experience of every one of them contradicts, “ THAT THERE IS
“ AMPLE WEALTH IN ENGLAND READY UPON THE SLIGHTEST
“ HINT TO SUPPORT THE EXPENCES OF SCIENTIFIC INVESTI-
“ GATIONS.”

The last subject noticed by the author of the pamphlet under review, is the subject of conferring titles on persons eminent in science and literature. On this subject the author substantially agrees with Mr Babbage, but he is so unwilling to entertain one opinion in common with him, that he contrives, even on this point, to fall out with him by the way.

“ Among the means” says he “ of encouraging science in
“ England, Mr Babbage proposes the institution of ‘ an order
“ of merit.’ Does Mr Babbage imagine that Dr Wollaston or
“ Dr Maskelyne, or some such men, would have been more re-

“spected either at home or abroad, if, like the Russian, they
 “had half a dozen of different coloured ribbons pending on his
 “breast? In almost every country where such distinctions exist,
 “one-half of the ribbons are given to jobbing and patronage;
 “and upon the whole, we believe that the institution itself has
 “an injurious tendency. Such crosses and badges are but too
 “often the price for which honour and conscience are bought.
 “These gaudy baubles are the hooks and baits by which a prey
 “may be allured, which could not be taken in any other way.
 “The distribution of these distinctions must of needs belong
 “to government. How are its members to judge of the differ-
 “rent degrees of scientific merit? How can a minister know
 “whether an astronomer is a deserving honest man, or whether
 “he belongs to that numerous class of forgers, cooks, trimmers,
 “&c. whom Mr Babbage so justly brands and vows to execra-
 “tion? Mr Babbage does scarcely allow to government the ca-
 “pacity of choosing its own scientific advisers,—How will it be
 “able to discern those who deserve the order of merit? A well
 “meaning government, not wishing to indispose any body, and
 “attaching deservedly no great value to these things, gives
 “a decoration for the asking, or for the slightest reason or re-
 “commendation. In this way such orders get at every body’s
 “button hole; and it is not extraordinary on the Continent,
 “to see a sleight-of-hand performer, a fiddler, or a mountebank,
 “decorated with some foreign order. Farinelli, a celebrated
 “*castrato*, was decorated by the king of Spain with the mili-
 “tary order of Calatrava; Napoleon gave the order of the Iron
 “Crown, *la couronne de fer*, to Velluti, another *castrato*.
 “The consequence of all this must be naturally, that in such
 “countries where such distinctions exist, fools are very eager
 “to have them, and really meritorious men do not attach the
 “slightest value to them, and very often refuse them when they
 “are tendered. Thus the late Professor Van Swinden refused
 “an order offered to him by the king of Holland.

“But in England there can scarcely exist any pretext for
 “instituting such an order; there is no necessity that govern-
 “ment should expressly allow certain persons the permission
 “to wear a toy attached to their button-hole. The king of
 “England, if he be inclined to science, and wishes to vest its
 “professors with distinction, may confer upon them the ho-

“nour of knighthood or of the baronetcy, as his late royal brother has done upon Sir Humphry Davy, and as he himself very justly did, upon Sir James South. There can be little doubt, if once the calculating engine is brought to perfection but that we are going to have a Sir Charles Babbage; and we must seriously lament, not for the sake of the men, or of that of science, but for the honour of government, that we never had a Sir William Wollaston, a Sir Thomas Young, and a Sir James Watt.”

In this passage our author completely misrepresents Mr Babbage's views. Mr Babbage *does not propose* an order of merit. He merely says, that it has been proposed, and he even states, almost in the words of his opponent, that if such an order existed, government would not be able to fill it properly in the *present state of public opinion respecting science, as it might be filled up through the channels of patronage, and by the mere jobbers in science*. When an order of merit has been mentioned it does not mean that there are to be ribbons and gewgaws dangling at button holes. It is meant merely to say, that an honorary title should be created for civil merit. Our author is mistaken, however, if he thinks, as he seems to do, that Mr Babbage and others have any desire for such titles. Respected as they know they are by the great and good men of every nation, they are satisfied that this respect cannot be increased by an empty and tinkling syllable prefixed to their names. They have spoken of the subject solely as an instrument for promoting science; and though they themselves, already in the meridian of their career, ask no reward for what they have done, and seek no stimulus to do more, they are yet sensible that such titles and rewards are powerful excitements to the young, and that philosophers as well as heroes may be formed by the ambition of honours that are within their grasp.

It has been more than once stated by our author, and we fear it is an argument addressed to vulgar apprehension, that science cannot decline in a country which is still adorned by many great names. We admit, and we do it with true pride, that the temple of British science still possesses some of its most ornamental pillars, but we look in vain for the massive granite which should fill up its noble outline. The ruins of material grandeur read to us the lesson of our intellectual decline.

The walls of our venerable fabric may have crumbled into dust ; the altar may have disappeared from its hallowed site ; and the ministering priests may have been enshrined beneath, while its pillars, like those of " Tadmor in the wilderness," may remain to attest in sullen grandeur that time has spared but its locality and its name.

ART. XXIII.—*A System of Geology, with a Theory of the Earth, and an explanation of its connection with the Sacred Records.* By JOHN MACCULLOCH, M. D., F. R. S., &c. In two volumes, 8vo. Pp. 512 and 483.

WE have not for a long time met with a book of more promise and less fulfilment than the one before us. It has neither the merit of strikingly original, though crude views, nor of sound unostentatious philosophy,—nor of a luminous exposition of facts,—nor of a happy effort of generalization,—nor of ability of historic detail,—nor, finally, least of all that it pretends to, of being a "System" fitted for the use of the student. It is also wanting in an eminent degree in that which may recommend even an indifferent work to the leniency of the reader,—its being written in a good spirit, and with a candid desire to do justice to those who have pre-occupied the same field. This very fact is enough to convey unfavourable impressions of the character of the work, which a more careful scrutiny will not perhaps tend to dispel.

The idea of a "System of Geology" at this day, without frequent and honourable mention of the names and labours of Cuvier, Brongniart, Humboldt, Von Buch, Sedgewick, Buckland, Murchison, Smith, and many others whom we need not attempt to enumerate, seems almost a contradiction in terms ; and as Dr MacCulloch confines himself in a great measure to the field of his labours in Scotland, we might more particularly have expected to meet with abundant references to the writings of Saussure, and Bouè, and Jameson, and Hibbert ; instead of which we find these two volumes absolutely innocent (we believe) of foot notes, without reference to the works, and, with a few rare exceptions, without mention of the names of these fellow la-

bourers on the field of geology, which many of them have devoted their whole lives to extend and enrich ; and it is painful to add, that in the few instances where such notices do occur, it is almost without exception, in terms of blame, and never, or almost never, in those of praise. Even with the want of this courtesy, or rather justice which we are entitled to expect, we might have been disposed to give the work due credit for its intrinsic merit—for views so great and absorbing as to blot out for a moment the recollection of the painful labours of immortal minds which had brought the science to such a point of perfection as to require only a master's hand to associate and generalize—or for a plan so compendious and didactic as should free the author from the charge of egotism, by the laborious and perspicuous selection of facts and views from every accessible source. The work certainly enjoys neither of these characteristics, and we suspect by this time, though we have seen no notice of it whatever, has taken its station in the ranks of literature,—a low one we believe, and above which it will never rise.

That the *System of Geology* has some few good points about it, no impartial person, we believe, will deny ; whilst its defects are so obvious and glaring, that men of far less intellectual stamina than Dr MacCulloch himself, will delight to tear it in pieces and trample it under foot, whilst few will ever attempt to separate the valuable grains from the overweeningly preponderating mass of chaff. That the Doctor does not even deprecate criticism is quite obvious from the whole tone of the work. He has fairly laid himself open to the most merciless attacks on every hand, and he will probably experience them. He seems almost to want his usual self-confidence in recommending his book : And how could it be otherwise ? During by far the greater part of his 995 pages, the matter is stale ; the manner is feeble ; the premises are often indistinct ; the conclusions are often inconclusive ; the matter is ill arranged, the views sometimes inconsistent or opposed ;* the work is ill printed ; it is devoid of illustration on wood, copper, or stone ; and the price is,—L. 1, 12s. This, we suspect, is the key-stone to the whole work ; the “*auri sacra fames*” peeps through at every corner ; and the Doctor, we imagine,

* Compare the last chapter with other parts of the work.

never counted strata with more assiduity in Argyleshire,* than he computed the multiples of L. 1, 12s.

We do not propose to enter minutely into the merits or demerits of the book ; but before making any extracts from it, we would point out rather a singular consideration which its perusal must suggest to a reflecting mind. We had occasion not long since to notice Mr Lyell's interesting first volume of "*Principles of Geology*," as the first attempt in this country for a long series of years to generalize upon the accumulating facts of geological science. His second volume is hardly in the press, when a work by Dr MacCulloch of similar pretensions appears, only with the more imposing name of a "*System*," having the same objects, professing the same end, and differing only in the mode :—but how different ! Certainly no two works on one subject ever appeared with an interval of half a dozen of centuries between them, more antipodal than these two at a distance of as many months. They coincide in few particular hypotheses,—in many they are utterly opposed ; but what is more extraordinary, they have hardly the title or subject matter of a chapter common to both. The bewildered reader, standing with an authority by no means destitute of weight on each hand, is apt to inquire where truth lies ? Whether the "*System*," or the "*Principles*," contain the essence of the science ; for it is utterly impossible that both can do so. It is really important to inquire into this anomalous condition of the science, (for, however triumphant each party may consider his own fundamental principles, and baseless those of his opponent, anomalous we must still consider it,) and to show how so important a schism could arise without throwing to the winds any imagined data which geology has been considered to possess.

We are prepared to concede that Dr MacCulloch possesses the advantage of a varied acquaintance with the physical sciences, to which many, or most of his opponents (and we include Mr Lyell,) lay no claim. This superiority, for such it undoubtedly is, may have done Dr MacCulloch more harm than good as a pure geologist, by the way in which he has employed it, and the disparagement which he has taken every occasion to throw upon the attainments of the modern school of geologists ;—yet it has led him to take some just views (as they ap-

* See the *System of Geology*, vol. i. p. 93.

pear to us) of the nature of the science, which have been wholly overlooked by some other writers, and which has led to an erroneous view of the qualifications requisite to the geologist, and of the great objects of his science to unfold.

Before geology had acquired "a local habitation and a name," the efforts of early speculators were directed, as is well known, to the wide and uncertain field of pure theory connected with the origin of the globe; the connection of scripturally historical with physical events, and especially the traces and effects of the Mosaic deluge. The descriptive portion of the science was then a blank. It would have been thought equally beneath the dignity of science to classify rocks and simple minerals, or to descend into a minute comparison of fossil organic remains with the existing species. Why should those to whom Nature has revealed her ample page scrutinize the particular mode of her operation, which could, they imagined, add little to the knowledge of general facts or laws? Why should the phenomena of active causes be examined, the destructive influence of volcanos and earthquakes, or the devastating operations of seas and rivers, when a comet might as readily be conceived to torrefy the terrestrial nucleus from pole to pole by the approach of its ignited mass, by its attraction to change the axis of rotation, and by the action of a vast tidal wave submerge the mountains, and raise to light and air the peopled cavities of the ancient ocean? No dynamical effect was too powerful for such resources, and as new phenomena arose, new under-plots were added to the drama of creation; new trains of wheels, no matter how cumbrous, were added without mercy to the old one, till the hands were made once more to tell true upon the dial-plate, and permitted to remain till new discoveries rendered new substitutions necessary:—

————— how they will wield
The mighty frame, how build, unbuild, contrive,
To save appearances.

An advancing state of physical science led men to amend the laxity which cosmogonical theories had assumed in the days of Burnet and Whiston, and the speculations of Hutton and Werner, both of whom gave much weight to facts of observation, raised a new and very superior class of geologists. What-

ever may have been the errors of these two theories, which assumed the names of their respective authors, it cannot be denied that, independently of any light they may have individually thrown upon the creative processes of nature, their influence on men's minds was highly important. The doctrine of consolidation by heat and pressure, and that of universal formations, were brought to their respective tests of experiment and observation; the former was confirmed as far, perhaps farther, than could have been expected, under factitious circumstances; whilst the other was shown to have originated in its utmost generality in the narrow views of the ingenious but untravelling projector of it; and thus men became habituated to the wholesome practice of using well their hands and eyes before they began to speculate upon the subject.

The acrimony which the Huttonian and Wernerian controversy excited, produced a reaction, and geologists resolved for a time to banish hypotheses and to endeavour to view nature as she was, untinted by the medium through which theorists are too willing to view the objects of their research. This most important stage in the history of geology, and the true bearings of which have not, we think, been sufficiently adverted to, has been so ably sketched in an admirable review of Mr Lyell's "*Principles*" in a contemporary periodical work,* that we shall make no apology for quoting it. We have no hesitation in referring for the paternity of the article to the banks of the Cam.

"Notwithstanding that the labour and industry of our geological observers have been such as men seldom bestow, except for the purpose of proving or confirming some favourite opinion, their self-denial and temperance in abstaining from theory-making have been quite as eminent as their diligence in collecting materials. To any one acquainted with the recent original productions of our geological literature, this must have occurred as a very curious characteristic. Nothing can be more copious or communicative than these publications, so far as details are concerned. To describe, to circumscribe, to subdivide the strata of particular localities; to class, distinguish, restore the animal remains discovered; to identify distant rocks in the minutest circumstances; these and similar subjects have been

* *British Critic*, Jan. 1831.

the theme of many a long and dreary communication which has been read to the Geological Society, and which it required all the ingenuity and good taste of the members to enliven by the agreeable extempore discussions which succeed the readings. In most cases the writers described what they saw, and that was all. Stones were to them as flowers to Peter Bell :

“ A primrose on a river's brim
A yellow primrose was to him,
And it was nothing more.”

“ The case was not as in former days, when the crystals of a single hand-specimen were supposed capable of revealing the history of the original condition of the world. No doubt among all this mass of detail, (most valuable and indispensable, notwithstanding its tendency to tiresomeness,) there did occur and must occur suggestions and opinions as to the causes and connexions of the appearances described. It was the glimmering of dim and distant truths of this kind, which often gave the subject the charm of an oriental fiction ; and lured the inquirers onwards,

“ O'er bog or steep, through straight, rough, dense, or rare.”

But that which was truly admirable, was the constancy and firmness with which our geologists abstained from either dwelling upon such speculations as important, positive, certain, and leading points in the science ; or from considering an assent to any such opinions as a proof of the orthodoxy and soundness of geological faith of any of their brethren. There was among them a strong suspicion of all generalities ; an inflexible rejection of every thing which referred to Mr Jenkinson's favourite topic of ‘ the cosmogony or creation of the world ;’ and a resolute determination not to be misled, even by the most tempting promises of a beautiful and consistent theory, into the rashness of generalizing from immature and partial observations.”

The singular discovery of the relation of fossil organic remains to the strata in which they are found, to which the beautiful collections of Woodward show that he had so nearly arrived, was reserved to immortalize the name of Smith. Such a discovery could not fail to absorb the attention of the embryo school of geologists who made the dogged determination to observe nature and nature alone ; and almost the whole weight

of talent devoted to the rising science was embarked in this one research. The superposition of rocks, their connection, subdivisions, and classification, were all studied by the new light of fossil remains: the broken shell, or the mammoth's tusk, which a century ago would have been referred to as a proof of the deluge, (and in saying so it would have been considered as the sum total of information which the fossils were calculated to afford,) is now regarded merely as an index to mark the formation in which it is found, as occupying the place of a given term in a known series of rocks, or rather a series of synchronous deposits; and the knowledge thus obtained would be regarded as amounting merely to the determination of the structure of a given spot upon the surface of the globe, without reference to any ulterior or speculative question. Nothing, therefore, can be more distinct than the character of geology in the three periods we have sketched; and it is worth inquiry whether the science does not admit of a division of objects equally distinct. We have had the cosmogonical or earliest period, the middle one, or period of theoretical induction, from observation and experiment more or less extended, and the last, or age of purely descriptive geology.

The course pursued has been precisely the reverse of that which has obtained in the exact sciences. Men began with the transcendental and advanced to the rudimentary. The first geologists elaborated their systems in their closets, the next race combined the facts of nature in the most philosophical manner that their abilities or opportunities permitted, and deduced theories, legitimately so called; the last do little more than collate their note-books,—give a natural historical account of what they see,—identify their shells and plants with Sowerby and Brongniart,—and deposit their insulated facts in archives which are to be the mines out of which a future and happier race of philosophers are to draw conclusions and build hypotheses. If such be admitted to be a correct picture of the present state of geology, we can only congratulate its future cultivators upon having such indefatigable and disinterested predecessors, who are willing themselves to go into eternal oblivion, provided the materials they have spent their lives in quarrying are destined one day to form a portion of a great and symmetrical edifice. This we know to be the view of some

of the ablest and most long-sighted of those now engaged in this gigantic labour.

If we have succeeded in explaining ourselves, it will be very clear that we would wish to consider geology as having two essentially distinct parts,—Descriptive Geology and the Philosophy of Geology. In fact, the mere description of the earth's crust is essentially a branch of natural history, whilst the speculation becomes one of physics the moment that the element of *time* is introduced, and we begin to investigate the history and causes of any species of change to which it may be subject, whether past or future. This, then, is a clear line of demarcation; and the two branches of the science are so necessarily distinct, and require such a very different class of acquirements and habits of thought for their respective prosecution, that we wonder they should so generally be considered as one and the same, or as distinguished by a line so purely metaphysical that the one cannot be practically cultivated without the other. For ourselves, we have no hesitation in considering the physics of geology as the most interesting and important department of the science, without any wish to undervalue the practical labours of those who furnish data to found and to test speculations. But are we to infer that men wanting the technical knowledge requisite to the complete natural historical description of a phenomenon, should be the less able on that account to deduce correct inferences respecting its causes and the legitimate conclusions to which it leads? Or is the relation of the mere observer the less to be relied on because he may be incapable of drawing from it all the inferences it may warrant? We would say, much the reverse. The theory of the one and the facts of the other are only the more worthy of confidence; the one is unable to bend his facts to accommodate his theory; the other has no theory to support. To take an example: A physical geologist wishes to know the epoch of elevation of a mountain range; and suppose that from the direction of the chain he suspects from hypothesis that it was elevated at one period rather than another; he finds one rock formation lying upon the flanks of the range in a way indicative of having been raised along with it, whilst another is superimposed unconformably in horizontal strata. He is entitled to infer from *dynamical* principles that the epoch he re-

quires lies between the date of deposition of these two rocks. Ignorant of the niceties of discrimination of fossil species, or possibly, of mineral character, he refers to the descriptive geologist to assign the respective places of the two rocks in the system, who may perhaps know nothing of the purposes which this determination is to serve. Is the doctrine of parallelism of chains of synchronous elevation more or less satisfactorily determined by the co-operation of these two observers than if one only had been employed? It were easy to multiply illustrations.

Considering the vast multiplicity of sciences, many of them burdened with enormous technical labour, which become necessary to a complete geologist, it were vain to look for such a "perfect monster." Mechanics, dynamics, hydrostatics, pneumatics, chemistry, mineralogy, botany, zoology, are but a portion of the encyclopaedical knowledge with which he ought to be familiar, several of which individually become the labour of a lifetime fully to master: Hence a certain subdivision of labour has been successfully resorted to, and even the natural historical school of geologists often depend for their technical facts upon the authority of a single individual who has studied the subject. The moment a doubtful fossil is presented to the geological society, if it be a bone, it is put into the hands of Mr Clift; if a shell, Mr Sowerby gives his verdict; if an insect, Mr Kirby; if a plant, Mr Brown or Mr Lindley. This is as it should be; but in the higher divisions of the science a similar plan should be followed; the physics of geology should not be made *subordinate* to, but *distinct* from pure natural history. The ponderous and most elaborate volumes of the *Geological Transactions* contain piles of facts, but the spirit in which they are treated seems to indicate a trivial standard as the ultimate aim of the geologist. To determine to which subordinate stratum of a large group a particular rock is to be referred, is often considered a most important question as a matter of fact, yet it rarely happens that such a determination is capable of leading to the revelation of a class of general laws. Though we had a perfect geological map of the globe, the minds which were engaged in its construction might be quite incapable of drawing any valuable conclusion from it, and in precisely the same way we might consider the author

of the most elaborate paper in the *Geological Transactions* as incapable of writing either the elements or a system of geology. He might give lists of characteristic fossils,—he might teach the mode of observing, collecting, classifying rocks,—he might give the whole substance of Conybeare and Phillips's *Geology of England*, and yet might prove himself incapable of enunciating a single general law of the science, of drawing just geological conclusions, of assigning causes adequate to effects, or commit errors which the humblest elements of dynamics, hydrostatics, or chemistry would have prevented.

That many important facts depend upon apparently trivial determinations of the geological position of individual strata, we must not be conceived to deny. All we assert is, that such a determination, *per se*, is of little interest to science, and that much precious time, capable of giving the most important extension to our knowledge of natural laws, has been spent in the determination of matters of fact which, the chances are equal, may never add to the limits of geological science. To well directed efforts we can set no limits. The physical geologist must be perpetually indebted to the natural historian for the determination of facts which the knowledge of the latter will enable him to answer much more decisively than any moderate acquisition of the former could be expected to effect. Yet the mechanical philosopher is not a closet geologist. His department does not, indeed, lead him to map the junction of two strata with the precision of a surveyor, or to decide with elaborate investigation whether a red sandstone be old or new, or a limestone represent the encrinural series or the coal measures:—but his walk is by the volcano and the torrent:—It is he who can judge how far causes now in action are competent to the production of past events. He watches the silent erosion of the ocean-beaten cliffs; his skill computes the slow but sure transportation of the glaciers. Trained in the school of sober induction, he passes with philosophic caution from the known to the questionable; he traces the analogy of present effects to past effects, and thence, of present causes to past causes; he can seize the differences which science teaches should subsist between the results of power small in amount prolonged through time of indefinite extent, and of power indefinitely great acting during a short space of time. The mineralogist may compare

the volcanic and trap rocks, and pronounce their natural-historical character identical; but how infinitely more general and conclusive the deductions of the geologist, exercised by habits of physical research, in pointing out the real discrepancies of aqueous and igneous solution, the chemical effects of heat on rocks, the proofs of mechanical force in elevation, the testimony which the now rigid mass can be made to reveal of the direction and intensity of the effort.

Need we touch on the loftier speculations connected with geology:—Of the speculations which the figure of the earth renders more sure than almost any others to be found in the science, which yet, from the want of the requisite knowledge, few geologists can do any thing but take upon trust:—Need we allude to Mr Herschel's inquiries into that portion of geological dynamics connected with astronomy,—thus providing a link between the revolutions of our globe, and those of the system of which it forms but a single member? When we consider the necessary difference, we had almost said opposition, which subsists between the pursuits of natural history and those of experimental philosophy, can we wonder at the want of tact in observing, (or rather in treating observations,) the illogical reasoning, the inadequacy of causes to effects, the arguments *a priori*, the bigotry to hypotheses, which so frequently characterize the writings of mere descriptive geologists; and we must consider that the science rests on a tottering basis till some one sufficiently qualified devotes himself to the physics of the question, and places it on a better footing. But the rage for technicalities, which gives the present geological school its peculiar character, which at little expence of thought occupies the physical and mental energies to a certain extent, and permits the imagination to run wild in its most fantastic course, whilst an error in nomenclature is the gravest offence it recognizes,—so long as this rage continues in its full extent to divert the mind from the whole to part, and to make natural history the first, second, and third, requisite to geology, we can hardly hope that those who might work a revolution in the science, will be induced to set their face against the “exclusive” habits of the age. But we believe a day must come before geology is altogether worthy of the name of a science, when men, (either in this or some other country,) worthy of

the names, and inheritors of the talents of Davy, Wollaston, and Young, may be equally identified with geology, as the brightest of those who now adorn and uphold its descriptive department.

That some of the modern school of geologists have exhibited acquirements of both kinds in an eminent degree, we should be the last to deny ; but upon the principle that solid discovery is most likely to attend the efforts of those who circumscribe their energies to a narrow track, we believe that the true interests of science would be better served by some such discrimination of objects as we have pointed at, than by an attempt to grasp at such wide and varied qualifications as belong to the *beau ideal* of a perfect geologist. *

It is easily conceivable, upon the principles now laid down, that two works might be written upon geology, each sufficiently complete in their respective departments, yet with hardly a subject in common. From the scientific school in which he has been trained, the strongest part of Mr Lyell's book was, as might have been expected, what belongs to natural history ; and we formerly took occasion to express our opinion of the weakness of the physical department of the book,—an opinion which has since been amply confirmed in other quarters. But Dr MacCulloch, with a store of knowledge which well fitted him for entering upon some parts of this wide field, has shown

* We believe we run little risk of making an invidious comparison by saying, that perhaps the fittest man in Britain for writing upon geology at large is Mr Conybeare. His acquirements in the exact sciences and in general physics, and his stock of knowledge upon the details of geology, to which he has so largely added, fit him peculiarly for the task ; and we hope he will be prevailed upon to give to the world a compact system, instead of continuing the detached fragments which for some time appeared at monthly intervals in the *Philosophical Magazine*. We may add, that in Mr De La Beche's *Geological Manual*, which we have only had leisure to look into since the present article was written, we can perceive, amidst the superabundance of natural-historical details, a just sense of where the distinction of physical geology ought to lie. The "*Sections and Views of Geological Phenomena*" of the same author, are most valuable ; and, as their name expresses, abound in examples of those inestimable traces which nature sometimes leaves of her *modus operandi*,—traces which it should be the principal object of the physical geologist to discover and investigate, and which the mere natural historian may often pass over without a moment's observation.

a similar mistake in attempting the whole, and in a spirit ill adapted to soften the ruggedness of the real deformities of the work. With that propensity we so often see to view that side of a subject as all-important, of which our previous knowledge enables us to form a just idea, and to consider every other point of view as insignificant, Dr MacCulloch uncompromisingly upholds every tittle of the supremacy of mineral character, and with a levity which is quite characteristic of his style, throws aside, as utterly worthless, the whole theory of fossil remains, as indicative of the strata. He exalts the primitive rocks to an unmeasured degree of importance over the secondary and tertiary in the very spirit of Werner, whom, nevertheless, he takes every opportunity to discredit and trample on, and to whom he bears the farther resemblance of drawing almost all his illustrations from a country, limited both in absolute and geological extent,—namely, Scotland. It is not wonderful that the Doctor should choose this country for the Saxony of his “System.” There are few countries which ever *repaid* so well the labour of geological research, and few observations have ever been published in such a variety of form as Dr MacCulloch’s. It must be unnecessary even to refer to a certain Parliamentary return upon a “Mineralogical Survey,” to prove that modern alchemy has achieved the transmutation of even the basest minerals into gold,—that the philosopher’s stone may be discovered by the adept in rocks of every age:—

“Tollit humo Saxum ; Saxum quoque palluit auro.

Contigit et glebam ; contactu gleba potenti

Massa fit.”

Ov.

With regard to the volumes before us, we have neither space nor inclination to enter into any analysis of them ; and they possess few either of the charms of style, the merit of novelty, or of any other kind, to tempt us. The work is, to speak geologically, a conglomerate ;—not, however, composed of the varied materials which have so long been added as insulated groups of facts to the science,—not gleaned with fidelity and judgment from the works of predecessors and contemporaries, and set in a basis which should at once compact and add lustre to the whole ; but it is a conglomerate of the triturated and time-worn fragments of the Doctor’s own works, and the “basis or ground” is of the same sombre hue with the morsels which it

encloses. There is hardly one of the thousand pages in which Dr MacCulloch does not implicitly or explicitly appear; and the Account of the Western Isles of Scotland, the Classification of Rocks, with his scattered papers in the *Geological Transactions*, (which by the way he never quotes but to mention his own memoirs, unless in terms of contempt,) down to the very Description of Hammers in the Appendix, and other gleanings from his contributions to periodical works:—these are the staple commodities of the book, which are now piled together with so little arrangement, that the Doctor openly professes to express different opinions in his second volume from what are contained in the first; and the very arrangement of the chapters, (like dislocated strata,) assumes a different configuration in the body of the work, and in the table of contents.

We are at a loss where to quote from: The preface, perhaps, presents as good an idea of the style and pretensions of the work, as any part of it. We shall therefore make one or two brief extracts.

Of all announcements by way of recommendation for a work on a daily widening science, the following is the strangest we have met with: "This work was written in 1821 (!) and it has therefore slept even beyond the Horatian period." The succeeding paragraph throws completely into the shade anything we have attempted to say respecting the neglect of the physical department of geology. "I have waited ten years, in the announced hope that some better man would stand forward in the breach, the representative of Geological science as it now is: endeavouring also to tempt others to this perilous honour, by the occasional publication of certain portions. I do not see that it has been superseded; nor that there are any hopes from a longer delay. I did also expect that time would have brought material improvements, through the efforts of the present multitude of labourers. I grieve to say that it has scarcely received a valuable addition from this source, and not a single fundamental one. The evidences of Geology have indeed been multiplied; yet through identical facts only: since I do not perceive that a new one has been added to the science. This ought not to have been."—Pref. p. v. The modest author's idea of his work may be gathered from

the following passages. “ If, to controvert, be termed controversy, I am sorry for what I could not avoid. I neither envy the taste nor the feelings that delight in this bane of modern science and literature : under which, they who can contribute nothing, seek for fame, by depreciating what they even rarely understand. Never has there been a science, unless it be physic, so encumbered with rubbish as geology : it was impossible to move a single step without clearing it away. He who desired to build on the solid rock of Nature could not but attempt to remove the ruins that obscured it. And whoever seeks to ‘ make his understanding a repository of truth for his own sake, rather than the warehouse of other men’s false and inconclusive reasonings,’ will follow the same plan.

“ Of the arrangement of this work I have little to say. I have referred to my Classification of Rocks as the grammar of this science, while avoiding, as much as possible, all collision between them. If the order of the subjects does not prove satisfactory, I will gladly hear of a better ; yet it must be from one who has bestowed equal thought on an unexceptionable arrangement. If there are repetitions, and if references to things not yet examined, I shall be pleased to see any plan that will remove this blot from one place, without leaving an equal or worse one somewhere else.”—Pref. p. vii.

“ I am sensible that the sketch of a Theory of the Earth requires a volume, instead of a Chapter ; and further, that it can scarcely be understood without that series of drawings, as a guide through a perfect labyrinth of reflections, without which I could not even have written it. It must remain for others to demand such a volume : and if it is not less true that illustrations would have been most useful to the whole work, as they had been prepared for it, there are few who do not know where the obstruction lies, as it is they alone who can remove it.”—Pref. p. viii.

This is really too bad, in a thirty-two shilling book, without (as we have already said) even the semblance of a wood-cut, or illustration of any kind : but we can heartily subscribe to the assertion, that the “ Theory of the Earth” is greatly in want of some such elucidation. Would not the India Company’s 100 copies have paid for a few diagrams ?

Take as a specimen of the application of science the follow-

ing lecture on hammers: "The hammer of quarrymen is a double truncated wedge: while nothing but extreme ignorance of mathematical principles could have extended this form to those of road-makers and geologists: whence their inefficiency, even under an inconvenient increase of weight. The fragment, in each case, is produced by exciting a vibration in an imaginary lamina, through a motion too sudden to be accompanied by that of the adjoining parts. It is forgotten that the communication of motion is not regulated by the momentum, simply, of the moving body. The weight and the velocity cannot be indifferently interchanged: and thence strange errors in many other matters also, of daily practice. But, not to enter further on this subject, suffice it, that it requires a time inversely proportioned to the tenacity of a body, to allow it to be displaced in a mass; whence, if additional momentum be required, the weight, not the velocity, must be increased. But if it is required to disintegrate the same body, it is the velocity that must be augmented. It is by impulse that the stone is broken in the present case. Hence it is useless to increase the weight of such a hammer, beyond that to which the hand can give the greatest velocity, since there is thus a loss of power. I have taken four pounds as the extreme weight; while two, or less, will, in a proper form, suffice for most rocks.

But that form depending on the mode in which rocks split, the whole impulse should fall on as narrow a line as possible. In practice, however, that cannot be; because the centre of percussion of a wedge cannot easily be made to impinge vertically on a surface. But the same object is sufficiently attained by the impulse on a point, through a spheroidal form: as, in a sphere, it is evident that every direction will be, equally, and always, fully, effectual. But this being difficult to construct, is also unnecessary, while the oblate spheroidal form is even better; since the curvature of the equatorial portion allows of a narrower point of contact, with an equal weight than a sphere would do: the breadth of that contact diminishing the power, for obvious reasons."—Vol. ii. pp. 466, 467.

The chapter on the "Relations of Organic Remains" is perhaps the most extraordinary and intemperate part of the book. But to do Dr MacCulloch justice, we coincide much in the following views with which it opens: "The increase of knowledge has given a very different complexion to this subject,

and a more rational direction to the pursuit. Yet the geologist seems in danger of forgetting that it is but one part of his science. Its details belong to zoology and botany; and he loses sight of his main object when he pursues these minutiae to the neglect of their more interesting connections with the history of the globe. Still more deeply does he err, when he imagines that a theory of the earth can be founded on what involves so small a portion of its structure and history. It is doubtless, essential to know these objects; as, to arrange and name them is the grammar of this department. But it is unfortunately true, that whether the contemplation of minutiae disables the mind for wider views, or that only a minute mind can be engrossed by such things, the power of profiting by collections and their study, diminishes in proportion to their extent and the activity of collectors, whether it be in natural history or books.

“The true business of a geologist, here, is a of a far higher character. It is to determine the antiquity of these objects and that of the earths in which they lived, the waters which they inhabited, and the former places of those; to explain why they are now imbedded in rocks when once free, why elevated on the land when once beneath the sea, why they are partially distributed, and far more; as it is also his office to see how these things explain the history of the earth. If found in alluvial soils, other inquiries of an analogous nature arise, relating especially to the later history of the globe. And in the study of the objects themselves, if he undertakes the office of the zoologist and botanist, it is his business to compare the dead with the existing races; through which it is his own proper office to draw inferences as to the history of the living creations of the earth, as to that of the earth itself.”—Vol. i. pp. 406, 407.

The views of climate in this chapter, (Vol. i. p. 432, &c.) which do not admit of abridgement, are vague and most unsatisfactory, endeavouring to prove that we have no evidence of a change of climate.

Perhaps Professor Sedgewick and Mr Murchison could tell us something about the following passage: “The evidence for the existence of orthoceratites abundant in a quartz rock in Sutherland, which follows gneiss and is connected with it, I consider to be as perfect as we are entitled to expect on such a subject,

from the researches of one individual, under the neglect of all other observers. In any case, where no previous hypothesis to the contrary had existed, no one would hesitate in receiving it. "If a recent geologist has undertaken, in the last case, to explain otherwise what he never saw, because he preferred ancient ignorance to modern truth, (!) he has also forgotten that the secondary and the primary strata never can undergo a joint undulation: he could not have been ignorant to this extent, though his researches had been confined to the recent strata: it has been the oversight of anxiety for an hypothesis. And geologists would be better employed in seeking for new truths, than in determining what is, because they think that it ought to be. If this is to be the rule, all observation is useless: he who has seen nothing becomes the interpreter of everything, and interprets as he had previously statuted to do: Geology is attained, and we may rest from our labours. But if it is thus to become a science, it is the first that ever succeeded through this road. I owe this remark to the geological student, (!) whom I have undertaken to teach in all that I know myself, and whom it is my duty to guide where he has been misled."—Vol. i. pp. 418, 419.

We conclude with Dr MacCulloch's memorable final paragraph on "the identification of strata through organic fossils," which we suppose he considers fine writing; if so, we imagine he stands alone. "It is quite time that geologists should review their system, in this, as in far more, and correct their practice. Then, and not till then, shall we begin fairly to study the history of the Earth; and, in time, we shall know the Truth; the Truth itself, and the ever odious truth that we have been in the wrong. Let shells be used as aids, as far as they can: but if strata are to be determined by shells alone, it will be the land of shells and conchologists that will set the pattern of the earth, as other lands and other conchologists, will, under Breislak's remark, contest for other patterns. This is not geology; and thus will Geology never be."—Vol. i. p. 454.

We shall not attempt to criticize this extraordinary chapter,—we leave it in far abler hands,—to those by whom it will be thoroughly sifted,—to the dissecting knife of the comparative anatomist, and the tender mercies of the members of the Geological Society.

ART. XXIV.—REGISTER OF THE BAROMETER, THERMOMETER, AND RAIN-GAGE, kept at Edinburgh. By ALEX. ADIE, Esq. F.R.S. Edin.

The Observations contained in the following Register were made behind the Regent Terrace, on the south-east bank of the Calton Hill, by means of very nice instruments, constructed by Mr Adie. The height of the instruments is feet above high water-mark at Leith. The morning and evening observations were made about 10 A.M. and 10 P.M.

JUNE 1831.

JULY 1831.

AUGUST 1831.

Day of Month.	Thermometer.			Register Therm.			Barometer.		Rain.	D. of Week.	D. of Mon.	Thermometer.			Register Therm.			Barometer.		Rain.
	Morn.	Even.	Mean.	Min.	Max.	Mean.	Morn.	Even.				Mean.	Min.	Max.	Mean.	Morn.	Even.	Mean.		
1	61	57	60.5	48	63	56.5	29.94	29.90		F.	1	68	58	63	57	68	62.5	30.03	30.04	
2	68	55	61.5	52	72	62	29.98	29.81		S.	2	64	58	61	56	66	61	30.00	30.00	
3	58	53	55.5	48	62	53	30.08	30.11		S.	3	60	59	59.5	56	62	59	29.96	29.83	
4	64	57	60.5	47	71	59	30.06	29.93		M.	4	62	60	61	58	63	60.5	29.93	29.61	.04
5	58	51	54.5	49	61	56.5	30.08	30.04	.06	T.	5	67	59	63	58	67	62.5	29.58	29.48	.11
6	51	49	51.5	43	58	51.5	30.08	30.07		W.	6	68	60	64	58	70	61	29.48	29.48	.11
7	51	35	51.5	43	58	51.5	30.08	30.70		M.	7	62	58	60	56	72	61	29.58	29.70	.65
8	59	56	57.5	46	63	55	29.60	29.68		F.	8	63	61	65	54	71	62.5	29.67	29.75	
9	58	56	57	45	63	55	29.60	29.47		W.	9	66	68	67	58	76	62	29.78	29.36	
10	60	55	57.5	51	61	56	29.40	29.40	.36	T.	10	62	62	62	56	72	61	29.78	29.36	
11	57	60	58.5	51	63	58	29.58	29.50	.10	M.	11	63	63	63	56	71	63.5	30.01	30.00	
12	62	60	61	51	69	60	29.60	29.63		T.	12	68	58	63	56	71	63.5	30.01	30.00	
13	66	57	62.5	53	70	62.5	29.68	29.63		W.	13	58	61	59	53	67	60	29.94	29.86	
14	67	57	62	54	70	62	29.82	29.75		T.	14	62	59	60.5	52	66	59	29.87	29.90	
15	63	57	60	51	66	60	29.56	29.46	.10	F.	15	64	57	60.5	54	68	61	29.90	29.97	.85
16	66	55	59.5	52	66	59	29.42	29.50		S.	16	65	50	61	51	67	60.5	29.98	29.98	.07
17	63	57	60.5	51	62	58.5	29.53	29.55		T.	17	60	56	58	54	65	58.5	29.94	29.87	
18	64	57	60.5	51	66	58.5	29.61	29.55	.08	M.	18	60	50	55	48	62	53	29.68	29.55	.37
19	62	58	60	52	66	58.5	29.57	29.75		W.	19	57	57	57	53	65	59	29.45	29.62	.10
20	60	54	60	52	68	60	29.88	29.83		T.	20	60	59	59	53	66	59.5	29.87	30.12	
21	63	55	60	51	67	60.5	29.83	29.93		W.	21	63	59	62	53	65	58	30.22	30.20	
22	63	54	58.5	50	67	58	30.03	30.10		T.	22	65	60	63	54	74	64	30.13	30.08	
23	63	55	58.5	50	66	58	29.98	29.73	.17	F.	23	55	55	55	55	70	62.5	29.97	30.73	.04
24	60	53	56.5	51	61	56	29.55	29.66	.22	S.	24	55	55	55	55	67	62.5	29.47	29.34	.10
25	54	50	52	45	59	52.5	29.59	29.51	.11	M.	25	58	58	58	55	63	59	29.55	29.46	
26	51	45	48	43	56	49.5	29.63	29.70	.07	T.	26	60	57	61.5	55	65	60	29.46	29.47	.18
27	50	43	46.5	43	56	49.5	29.67	29.81		W.	27	60	56	58	55	65	60	29.60	29.63	
28	52	35	43.5	32	63	58.5	29.90	29.81	.06	T.	28	58	52	53	50	60	55	29.70	29.82	.20
29	59	57	58	49	69	59	29.88	29.89		F.	29	63	53	58	52	63	56	29.80	29.80	
30	65	53	59	52	71	61.5	29.92	30.05		S.	30	58	50	54	50	65	56.5	29.52	29.70	.107
Sum.	1856	1650	1743	1309	1975	1742	892.90	893.17	1.41			1931	1775	1861.5	1675	2035	1865	922.62	923.05	1.53
Mean.	61.2	55	58.1	50.5	63.85	58.06	29.765	29.772				65.03	57.26	60.11	53.97	66.23	60.10	29.762	29.776	

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NOTICE TO CORRESPONDENTS.

MR MARSHALL'S Meteorological Observations, though in types, and other communications, have been again unavoidably postponed.

Fig. 1.

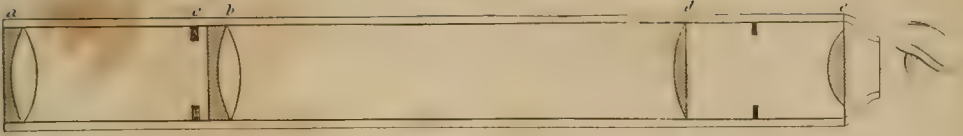


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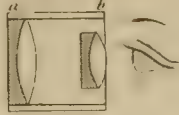


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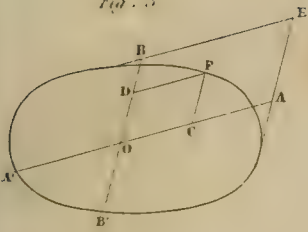


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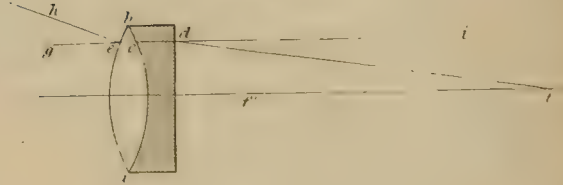


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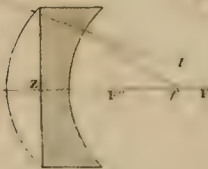


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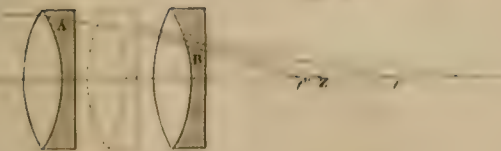
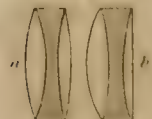


Fig. 7.



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Fig. 8.



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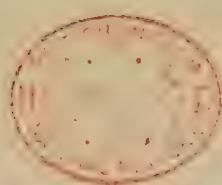




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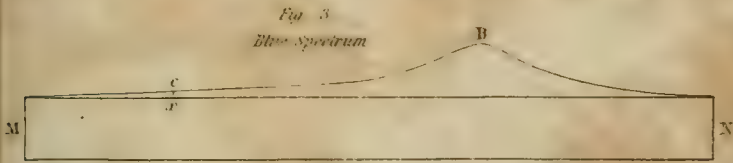
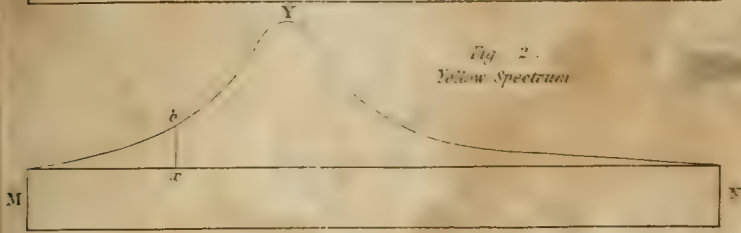


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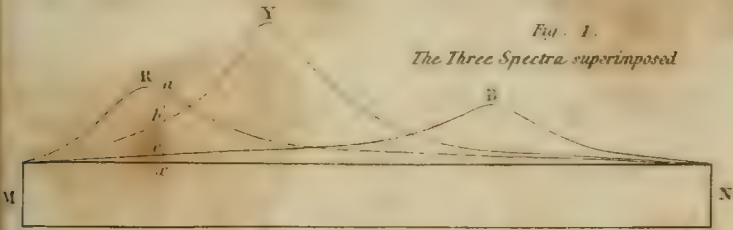


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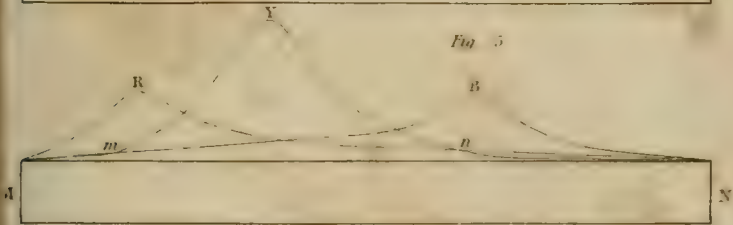


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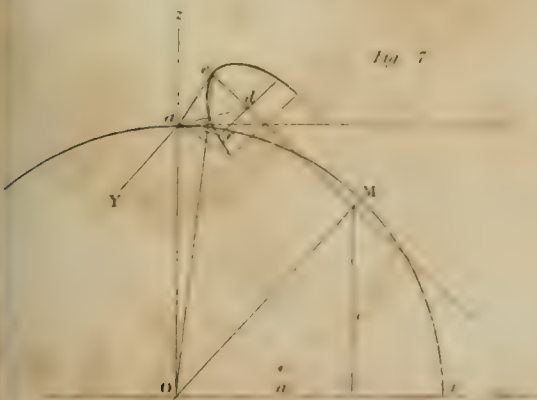


Fig. 6.



